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**Proceedings of the Princeton Workshop on
"New Approaches to Experimental Turbulence Research"**

Held at Princeton University

September 5-7, 1990

Supported by AFOSR

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Edited by

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**A. J. Smits
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Abstract

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INTRODUCTION

The concept of a workshop devoted to discussing the future of experimental turbulence research was first raised in the period immediately following the "Whither Turbulence" workshop organized by John Lumley at Cornell in March 1989. At that time, I had a number of conversations with Tony Perry (Melbourne University), Jean-Paul Dussauge (IMST, Marseille), and Hans Fernholz (Technical University of Berlin), and we agreed that, while Lumley's workshop had brought together a wide spectrum of workers in turbulence and had produced a very successful interaction among different groups with different philosophies, it had not addressed a number of important issues confronting turbulence research. First, the Whither Turbulence? workshop had been confined to discussions of technical interest, and it had not taken up wider issues that confront the turbulence community as a whole. The overall coordination of the turbulence research effort, the shrinking resources, the educational and faculty development issues, the lack of national visibility, were not on the program at Cornell. Second, Whither Turbulence? was mainly concerned with advances in the understanding of turbulence, and it did not address itself to the specific process by which we gain that understanding. In particular, the challenges facing experimental turbulence research were largely neglected.

When I contemplate the future of experimental work in turbulence, I see a crisis looming. Based on the experience of the past 5 to 10 years, I can only see further reductions in the available resources occurring at a time when instrumentation and facility costs are continually rising. We will inevitably be faced with some difficult choices: will we be able to continue to maintain high quality experimental work when financial pressures will restrict our efforts to a small number of inadequately equipped table-top experiments, or will we be willing to participate in research activities concentrated at a few well-funded, well-equipped research centers? How can we expect the newly-appointed assistant professor to build an active experimental program in this atmosphere of shrinking resources and rising costs? Is it possible to expand the funding available for experimental work? How can we hope to investigate flows with, for example, high Reynolds numbers, compressibility, high Mach numbers, combustion, and heat release without new facilities and diagnostic techniques?

Almost by definition, we believe that turbulence is an important area for research. However, it is important in so many different areas that it's hard to see it as one unified subject. ASME turbulence is "different" from civil engineering turbulence and that is different from APS turbulence and AIAA has its own turbulence. Then there is the large meteorological community that also has a strong interest in turbulence. And that's how we act: we split turbulence according to our professional disciplines. I believe that this happens because there's a lack of recognition of turbulence as a big problem, a problem that transcends particular applications.

I believe also that it is because of the lack of recognition that we are seeing a decline in the funding area. I don't think anyone would dispute that turbulence research has become more expensive during a time when the resources are actually shrinking. It's also a contradiction that we can now do more than before with the new instrumentation, with the tremendous computers that are available to us - we can probably do more now than we can ever do and yet the resources are shrinking. I feel strongly that if we just let things go on the way it is going, things will only get worse. I hope that this workshop can contribute in

some way to reversing the decline that I see. Perhaps by working together instead of working as individuals we will achieve a sense of community in turbulence research. Recently, because of funding difficulties, I think we've often looked like sharks in a pool where someone is draining the water out. After a while we start eating each other. But we're trying to solve a common problem, and we should be acting together. At least that's what I feel, and it is that feeling of community engendered by our common aim that I hope helped to develop at this workshop.

Acknowledgements are due to Jim McMichael of AFOSR, Sharon Matarese and Diane Schulte for their organizational help, Princeton's Audio-Visual department, Jose Goldstein, Wolfgang Konrad, Kamal Poddar, Jon Poggie, Doug Smith, Randy Smith, and Mark Zagarolla, who operated the recording equipment.

Alexander J. Smits
Princeton, N.J.
June 24, 1991

WORKSHOP PROGRAM

"New Approaches to Experimental Turbulence Research"

McCormick Auditorium
PRINCETON UNIVERSITY
September 5-7, 1990

Tuesday, September 4

2:00pm - 4:00pm Setting up AV equipment: D.R. Smith, K. Poddar
6:00pm - 8:00pm Early Bird Welcome and Registration
Room D-225, Engineering Quadrangle: W. Konrad

Wednesday, September 5

8:15 - 9:00 Registration (outside McCormick Auditorium in the lobby
of the Art Museum): R.W. Smith

9:00 - 12:00 **Fundamental Directions for Experimental Turbulence
Research**

Chairman: F. Browand
Speakers: I. Wygnanski, J.L. Lumley, A.E. Perry
Session Recorder: T. Wei
Audio-Visual: M.V. Zagarolla, D.R. Smith

Lunch: Fine Tower

2:00 - 5:00 **Computers and Experiment: Data Sets, Data Analysis, and
Control. The Interaction Among Theory, Computation and
Experiment**

Chairman: A. Roshko
Speakers: B. Cantwell, S. Robinson, C. Smith
Session Recorder: R. Mehta
Audio-Visual: J. Poggie, K. Poddar

6:30 - 7:30 Reception, Prospect House
7:30 Dinner, Prospect House

Thursday, September 6**9:00 - 10:30 Instrumentation and Facilities**

Chairman: J.-P. Dussauge
 Speakers: L. Hesselink, R. Simpson, F. Browand
 Session Recorder: D. H. Wood
 Audio-Visual: M.V. Zagarolla, K. Poddar

11:00 - 12:15 Educational Issues - The Training of Graduate Students and Faculty Development

Chairman: H. Fernholz
 Speakers: W. George, R. Panton
 Session Recorder: D. H. Wood
 Audio-Visual: M.V. Zagarolla, K. Poddar

Lunch: Fine Tower

2:00 - 5:00 Strategies for Effective Turbulence Research in a Changing Environment

Chairman: D.J. Walker
 Speakers: J.M. McMichael, S. Lekoudis,
 K. Sreenivasan, G.L. Brown
 Session Recorder: E. Spina
 Audio-Visual: J. Poggie, W. Konrad

8:00 Working GroupsFriday, September 7**9:00 - 10:00 Reports from Working Groups and Summation**

Chairman: S. Kline
 Speakers: D. Dolling, D. Bushnell
 Session Recorder: J. Brasseur
 Audio-Visual: D.R. Smith, R.W. Smith

10:30 - 12:00 The Future

Chairman: B. Cantwell
 Speakers: K. Sreenivasan, J. Wyngaard
 Session Recorder: J. Brasseur
 Audio-Visual: D.R. Smith, R.W. Smith

SESSION 1

Fundamental Directions for Experimental Turbulence Research

Session Chairman: Fred Browand, University of Southern California

Session Recorder: Tim Wei, Rutgers University

Israel Wygnanski (University of Arizona, and Technion):

From the conditions that were posed to us in the written material, I understand that in this session, we are supposed to come up with ideas for future thrusts in experimental research in turbulence from both structural and technical points of view. There was an added constraint imploring us not to discuss our own research. I think that is a very important constraint, because it forced me to read foreign material and thus I started viewing the subject from a somewhat different perspective.

I decided to take a historical perspective in analyzing where we presently stand on the subject and where we might be going in the future. In that sense, the depth of the discussion will probably be reduced to a level used by commentators who discuss problems in which they themselves are not involved. I posed for myself a few questions.

First, is the study of turbulence important?

- if it is, how can we possibly improve our understanding of the subject matter?
- what is the role of the computer in the laboratory today and how we can possibly look at it in the future?
- how can we enhance the public acceptance and support?
- and what are the possible thrusts of the future turbulence research.

In attempting to answer the first question, I could mention many topics on which I am not personally doing any work, which reinforced my conviction that turbulence is really an important subject. We find turbulent flows wherever we look, and consequently we should try to control them to whatever degree possible. The understanding and the control of turbulence is important in improving the quality of our lives.

I had not realized the importance of turbulence research in attempting to solve cardiovascular problems. Not only is turbulence a major constraint in the design of heart valves, but it even appears that the wall stress controls the dynamics of vascular endothelial cells which apparently align themselves in the direction of the wall stress that they apparently sense. That alignment may have an effect on our health. If we would be able to promote the understanding of that small aspect of the field, we might be able to find cures

to as yet untreatable diseases.

Another example, from industry, to which I never gave a sufficient thought is that of a clean-room technology. It is very important in the production of silicon wafers, and it is totally dependent on our knowledge of turbulence. These are just a couple of examples, but I can go on and on citing examples from engineering.

How we will operate in the future? and how I would like to see future research in turbulence evolving?

There is a need for an interaction between theory, computation and experiment, and I can envisage this triad (Figure 1)* as being very important for the future orderly evolution of the subject. I can't talk much about a general theory, because to date, we did not come up with an all encompassing theory. It would be nice if we had concepts parallel to the concept of entropy in thermodynamics. But short of that, we have to exploit more the knowledge of the equations and of the boundary conditions. Therefore whatever model, of whichever level of complexity we choose to work with, it must be validated by making use of the two experimental techniques constituting the other legs of the triad. (I refer to the computer as a numerical experimental facility or rather to the Navier-Stokes simulator as to an experiment.) Each one of these two methods of experimentation has its shortcomings and its merits. For the numerical simulation we have an unlimited number of diagnostic tools; we can stop and analyze whatever results we get in any way we please. But we have to remember that it is an approximation to a physical problem because of discretization. We have truncation errors which have to be carefully treated and analyzed. We also have some difficulties with inflow and outflow conditions in open flow systems. In the laboratory, on the other hand, every new experiment involves a production of new hardware and the cost of such experiments may become prohibitive. In addition, the diagnostic tools, which are becoming increasingly more expensive, have to be somehow integrated with the hardware. So, each leg of the triad has its inherent difficulties, thus maximum efficiency of the system is achieved when all three become interactive.

Let us see how was this achieved in the past. Let us take for example the statistical theory of turbulence or a closure model of turbulence and see how it evolved. The Reynolds averaged equations had to be closed in some manner. Historically, we started with the simplest first order closure, and only after discovering its shortcomings proceed I to second order closure, then to the renormalization group analysis, and so on. We needed some inputs to complete the model so we resorted to physical experiment for the missing empirical data. The experiment supplied a vital input to the model which would have been useless otherwise and provided new ideas for improvement. The utilization of Reynolds averaged equations was not questioned till the mid-fifties, until Corrsin and Kistler discovered the superlayer. It became clear that turbulent flow could no longer be represented by splitting the equations of motion into two parts consisting of a steady mean motion and random fluctuations. Thereafter, a search for coherent structures began in earnest. In shear flows this search focused on the wall region of the boundary layer and on the intermittent region far away from the surface. However, we are not as yet capable of defining a universal direction for research on coherent structures and analyze our data accordingly. We do not possess an acceptable model which will supersede the statistical approach. There are models existing but they are not of a universal nature so we link many results to the Reynolds averaged equations and to statistical methods.

In the meanwhile, simulation techniques, consisting of various degrees of sophistication and

complexity have been developed. Simulation can be subdivided into categories like direct simulation, large eddy simulation, or simulation using Reynolds averaged equations-with decreasing degree of complexity. We used to test ad-hoc closure models by a physical experiment, we did not as yet validate our simulation schemes by checking them against one another. For example, if we use a model based on Reynolds averaged equations, can we supply the model with the same empirical constants obtained from other - say large eddy simulations as the experiment did in the past. We can thus provide another link within the above mentioned triad.

There is another activity which was developed recently that should be looked at. In this activity one considers turbulence as a black box to which a controlled input is added. One then observes the response of the flow-system to the controlled input. It is an important activity because it enables one to alter the flow system even without having a full understanding of it. If we would have a model for such a control, or a concept of some kind, we could perhaps do much better in understanding how we can control a given flow-geometry. Sometimes we may consider the flow from a control point of view only by providing arbitrary feedback control mechanisms. Although it is not pretentious it is a fairly noble activity which can be quite useful. Let me give you an example : This is a problem in combustion control investigated by Gutmark and his co-workers.

The sketch shown in Figure 2* portrays a flame which might be either a diffusion flame or a premixed flame. there is a detector by which one observes the light emitted by the flame and through it the condition of the flame. One continuously checks the light against an arbitrarily set point. One may feed the detected signal back into the system in an ,a priori known way in order to stabilize the flame. Whether one does it by using an FM signal, as suggested in the present example, or an AM control signals it is often done, is a particular aspect of the control theory. It is interesting to note that in this example there is a set range of frequencies which are somehow coupled to hydrodynamic stability which enables one to predict how to stabilize the flame. The results of such an experiment are shown in Figures 3 and 4 which were provided by Gutmark*.

These figures portray an AM control of a premixed flame. At a particular time the air flow used for the mixture was increased, and the flame was extinguished because the mixture became too lean. Externally exciting the jet forming the flame at a fixed frequency and amplitude did not help and the flame just went off. On the other hand, with the closed loop control of the same problem, the flame was sustained as may be observed from the luminescence emitted by the flame and recorded (Figure 4*). A very similar experiment involving the control of a wake was done by Dimotakis. In his experiment the difference between two pressure readings in the vicinity of the stagnation point was sensed and the cylinder was rotated to minimize the prescribed pressure difference. The effect on the size of the wake and on the eddy structure was most impressive. Thus, one may achieve an incredible degree of control over the behavior of a flow without a full understanding of the details.

What is the role of the computer in the laboratory?

What did we use the computer for? We mostly used the large storage capacity of the computer for data logging and processing. We could thus, analyze the same data in many different ways but we mostly use conventional statistical tools. We also started devising other diagnostic techniques but this venue was not exploited properly. Let me give you an example of a non-conventional way of data processing supplied by Ho and Huang. They used time

series digitized from a hot-wire signal and counted the peaks and valleys of each individual fluctuation of the signal. Using this information as well as the time elapsed between adjacent peaks they deduced the typical time scale of the small eddies. Having that and using Taylor's hypothesis, they found what is the length scale, and it turned out to be corresponding to the dissipation length scale obtained in the conventional manner. This method may serve to detect small scale transitions occurring in relatively low Reynolds number flows. This is just an example of the type of analysis which might be used in conjunction with a computer in the laboratory.

We could, of course, use the computer to control the flow as was already suggested. And there is another activity namely particle tracking and pattern recognition. This may eventually free us from obtaining quantitative data from a single point and provide instantaneous information over an entire field of flow. Such techniques require big computers and a massive amount of data. Computers may also be used to control and automate an entire experimental investigation. I have seen this being demonstrated on a fairly complex experiment which was remotely controlled. What can we learn from these developments?

First, we can take a cue from the procedural way in which computing evolved. I grew up with an IBM 620. We had to know the innards of this computer because we essentially had to use a machine language to program it (a language which was called SOAP). When FORTRAN (and the "new" IBM 1400 series of computers) came along we got removed one step away from the operational details of the computer. The big revolution came when the screen-monitor replaced punch-cards and people started to work interactively with the machine while being physically removed from it. How is large scale computing done today? Individual users have their pre-processors and post-processors at their desks and interact with them, while these processors communicate rapidly and efficiently with the large computing centers. Many of the centers are interconnected and the individual user obtains his results without knowing even where most of his calculations were done. The user by corresponding with the machine on his desks understands his program and his results intimately and this is all that is important to him. We may essentially do similar things in the laboratory and reform our outlook on experiments. Let me recapitulate how computations are done nowadays: we do small scale computations at our desks; larger scale computations in a computing center belonging to the department or the faculty; still larger in the computing center of the home institution, while only largest scale computations are done in some national centers.

The same operational pattern could take place in a laboratory because we can now control an experiment remotely. Some, small scale, exploratory experiments will be done by individual investigators as they were always done before. The more expensive, and the more elaborate experiments will be done in some sort of a center. And yet, by remote interaction in real time, we would be able to feel as if we were doing the experiment right at home without, perhaps knowing, or even feeling the need to know, the very details of what is happening. Take a wind tunnel as an example we operate it at home by having in it one experiment at a time. Suppose we get a larger facility with a lot of test sections which are made to be removable in and out of the tunnel's circuit. Every interested researcher gets a test section in which he constructs his individual experiment. Consequently everyone prepares his own experiment and has the opportunity of knowing it intimately. The facility as a whole is equipped with the best diagnostic tools and provides professional maintenance of those diagnostic tools in the same way that a computing center has professional people who maintain the computer. We are removed from the computer itself and we will be physically removed, in such a facility, from the experiment. Nevertheless, we can design an experiment the way we see fit, have it run professionally, and interact with the experiment while it is

being run - in real time. So there is the experimental analog, if you wish, to the computation procedure.

This approach to experiments in turbulence will certainly be much more efficient as far as the investment is concerned. And yet, it will enable us to expand the parameter space in which we carry out our investigations. Most university facilities are small and are very limited in terms of the range of Reynolds numbers and Mach numbers in which they operate. By having a cooperative (or a communal) facility the experiment may cover a greater range of parameters without losing the intimacy with the experiment.

Finally, such an arrangement may be beneficial from a pedagogical point of view. I found that a lot of my graduate students are spending more time with trivial details than with fluid dynamics and turbulence. They have to become experts on diagnostic tools, on computer systems, operating systems and a variety of instruments which get increasingly more complex with time. I think that this trend has to be reversed for students specializing in fluid dynamics.

* Figures not available at time of publication.

John L. Lumley (Cornell University):

First, let me consider where experiment stands relative to computation. They are, of course, not the same - one is real, and one is simulation. The initial conditions, for example, are never quite the same. Even when the results are superficially the same, there are probably subtle differences. Considering grid turbulence, however, the grid itself, which cannot be simulated in the computer, was an arbitrary engineering solution to the problem of how to generate a homogeneous turbulence without mean velocity gradients. A number of different grids were tried (square bars, round bars, biplane, monoplane, unidirectional, ...) and the turbulence produced were all different. It is a historical accident that we have more or less settled on the square bar, biplane grid with 1/4" bars on a 1" mesh. We can not simulate that particular flow exactly in the computer, but we can simulate homogeneous isotropic flows with other initial conditions. I feel that these other flows, though they are not exact reproductions of the physical flows we have come to know, have the same standing, and deserve to be considered as experiments of equal validity.

At the present time some flows are computable by exact numerical simulation (as opposed to large eddy simulation), although usually at relatively low Reynolds number. Extrapolating, it seems likely that within the next few decades computable Reynolds numbers will rise, and it will become possible to compute more complex flows, but it seems equally clear that in the foreseeable future many flows will not be computable. If a flow is computable, it is certainly preferable to compute it, rather than measure it in the laboratory, since the information obtained is far greater. There are still many variables that cannot be measured and we are limited to measurements at a relatively small collection of points, whereas in computation we obtain information on everything everywhere. In this connection, we may mention that many modern theories require a very great deal of information, and obtaining this experimentally, even if the experiment is controlled by computer, is often tedious, making a computation more sensible.

Secondly, I would like to say that whether an experiment is physical or computational, I believe it should be formed and guided and interpreted by theory. There are some problems with this. It is a little dangerous for the experimentalist and the theoretician to get too well acquainted - the measurements and the theory can converge on each other quite inadvertently. Also, a good experimentalist is fairly suspicious and disdainful of a great deal of theory, thinking of it as here-today-and-gone-tomorrow, and feeling it to be too conjectural to base an experiment on. He would like to think that his experiment has archival value, and should outlast these hokey, fly-by-night theories. But still, any good experimentalist, conservative though he may be, bases his experiment on some physical concept. An experiment that is not constructed around a theory is something like an exploratory operation: let's open the patient up and see if we can find anything. Theory is what gives meaning to things. Rutherford's words, quoted in the "Collected Thoughts on the Discussion Topics"¹ are delightfully provocative, and typical of an experimentalist, and I cannot disagree entirely, but I still have a serious quibble. We have the Navier Stokes equations, and we can compute some turbulent flows, but I think we would all agree that there is much that we do not understand. Understanding comes only when you have a

relatively simple physical picture of what is going on, that is sufficiently complete to permit you to predict what will happen with a certain resolution. There is, of course, a serious question whether turbulence can possibly be explained by any imaginable set of simple physical ideas, or whether it involves concepts that are not embodied in any existing set of these ideas, and will consequently require the development of new concepts. I believe that new concepts will probably be required. That does not change the principle. We can also ask what we mean by a "simple physical idea"; I take this to mean something that seems more or less irreducible, (although usually they can be reduced if you work at them), and that is very familiar. Often this is what we mean by simple, or physical, or even understanding: that we have seen the mechanism often enough that we are thoroughly familiar with its, and know what to expect in every conceivable circumstance, and believe we can see how these effects come about, the inner workings, so to speak.

Having said all of that, an experiment, or a computation, should be hung on such a theoretical structure, and the data interpreted in terms of that structure, in order to bring meaning, understanding, shed light. Negative experiments can also be constructed, to show that certain behavior is possible which is inconsistent with a particular physical picture. These are hung on a theoretical structure just as much as the positive experiments.

Now let me say something about fundamental versus applied research, and research planning. I think an applications-oriented environment is one of the most stimulating possible for both theoretician and experimentalist. The real world of engineering applications is rich in interesting problems. Many of these problems have a fundamental root; that is, in order to solve them, it is necessary to prune away irrelevancies until all that is left is the fundamental situation that displays the behavior that is not understood. This is the job of the fundamental research scientist. You can see that this work, although it seems ultimately very fundamental, is very practical, since the investigation has its source in the real world, and the results of the investigation will ultimately return to influence the real world. Of course, in the process of isolating the fundamental difficulty, other theories, computations or experiments may be required, that are not yet at the fundamental level. This is less fundamental, what may be called applied, research. In a similar way, when the fundamental experiment, computation or bit of theory has been done, it will be necessary to place the fundamental information that has been extracted in its practical context, show its implications for the real world, draw real conclusions from it. This is sometimes called technology transfer. All these parts are essential. In particular, we cannot exist entirely on the applied research, even if we are entirely on the applied research, even if we are entirely motivated by the desire to solve practical problems. We must prune the problem back until we have arrived at a fundamental level.

The United States is in many ways a particularly unfriendly environment for a fundamentalist scientist. We have a sociocultural/historical myth with which those of us who were children here grow up, of egalitarianism, practicality, inventiveness. An American, in this myth, is a man who rolls up his sleeves and pitches in, solving the problem at hand in a clever, simple, practical way, usually saying over his shoulder that he does not hold with book-learning. Edison is often suggested as an example. Many of our heroes had trouble in school. We tend to regard too much faith in what is written as being a foreign invention. This myth has a lot to be said for it as a model for raising a new country, and it certainly served us well for the first hundred years of the industrial revolution. I would not for a moment advocate abandoning it. However, American industry is getting some very stiff competition these days both from Europe and Asia. Some of the problems are related to the type of research that is being done by these competitors. (American industry, of course, has many other problems that are unrelated to its research base). I believe that in many cases

the products have gone beyond what can be fixed by Native American Know-How, and require quite sophisticated fundamental research. Our competitors have invested in such research for some time. American industry has resisted it (with a few notable exceptions). Nationally, we seem not to understand the necessity to reduce problems to their fundamental level, and are pushing more applied research.

So far as research planning is concerned, I am reminded of the New England town that had a bad fire, in large part because the hydrant was frozen. The town board passed a resolution that all hydrants should be tested twenty four hours before every fire. Certainly it is important to try to plan a research program, but you should not be misled into thinking that you can actually do it. We know what problems we think are important now, and we imagine we know what fundamental research will shed light on them. It is fine to plan to do that research. At the same time, you should recognize that historically much of the useful research was done for another purpose, or for no purpose at all. Hence, it is important that a broad range of research be supported. The time lag between a bit of fundamental research, and its application in practice, is of the order of ten to twenty years. If you do not support fundamental research now, your problems will not come home to roost for one to two decades, and you will probably not be able to alleviate them for another one to two decades.

Having said all that, let us get down to specifics. Laboratory experiments should probably stay away from most homogeneous, isothermal situations, unless there is some other complicating factor. Even simple stratified flows have been simulated (by Riley, and Schumann). However, for some time, at least, more complex stratified flows and flows containing particles, phase change, transfer of latent and sensible heat, chemical reactions with and without heat release, will mostly be beyond exact simulation, and hence good prospects for laboratory experiments. Many complex compressible flows are also too hard to simulate, although homogenous compressible flows (Erlebacher, Sarkar, et al) and compressible mixing layers (Lele) have been simulated, and Bogdonoff has told me of simulations of three-dimensional shock-boundary layer interactions. The trouble is, of course, that it is hard to design a truly fundamental experiment in a complex situation. Consider, for example, heat transfer in internal cooling passages in turbine blades, which is known to be unpredictably large, depending on the precise geometry, probably because of augmentation of turbulent transport but the interaction between the density differences induced by heat transfer, and the large body forces associated with the high rotational speed.

Presumably, an experiment of the future might make use of the most advanced technology. Rutherford's idea of simple experiments whose entire data output consists of "yes" or "no" is very attractive. Probably the nature of such simple experiments will not change. However, turbulence is an old field. A lot of those simple experiments have been done. We understand that a lot of what is going on, in a crude sort of way. What we cannot do is calculate, often because we do not have hard data, I am not saying that these wonderful, simple experiments are no longer possible, but that they are harder and harder to identify. At the same time there are other experiments that are necessary - data gathering experiments. Rutherford says we should let less clever people run behind us, collecting this data. Without speaking directly to this point, it is clear that someone must do this job.

In the area of Lagrangian statistics, for example, there remain many unanswered questions. There is precious little information on the shape of Lagrangian correlations, or the value of the Lagrangian integral scale, for example. From a practical point of view, better information on Lagrangian statistics would make possible better predictions of heat and mass transport in nearly all situations. As an example, we can explore an experiment in this area. Pope and his coworkers have done some very interesting things with Lagrangian analysis of

exact numerical simulations, but these are necessarily at quite low Reynolds number, and the flow must be forced at low wavenumbers to maintain it. Although I have no reason to believe that this low wavenumber forcing has infected the statistics, it makes me uneasy. Lagrangian transport is influenced primarily by the lowest wavenumbers, and it seems to me inevitable that the nature the energy input must influence the Lagrangian statistics. There are also severe problems with the growing errors associated with the interpolation necessary, since the Eulerian data are given on a fixed grid, and interpolation is necessary to obtain the Lagrangian path within the grid square. The time during which a material point can be tracked is severely limited. Hence, for several reasons this seems a likely area in which experiment can make a contribution.

The experiments are not easy, however. While it is possible to obtain particles that follow moderately high Reynolds number flows adequately (although the influence on higher order statistics of the small, but constant, slip should be analyzed), tracking them is another matter. A number of years ago, George Mellor did an experiment of this type. It almost has to be done in a tank, with a towed or dropped grid, so that the particles will stay in the field of view. Illumination is a serious problem - sheet illumination cannot be used, since the particles will move out of the sheet. It is difficult with classical illumination to get enough light to see particles that are small enough to follow the flow. More than twenty years ago (Snyder & Lumley, 1967) we used EG&G flash lamps, and these were barely adequate for a relatively small field. Now it seems natural to use high intensity laser illumination; a laser capable of producing regularly spaced, very high intensity, short duration pulses for a considerable period would be required. If laser illumination is used, then holographic images seem a natural - all planes would be in focus, and particles could be identified everywhere in the volume. Analysis of the images is non-trivial. The field should be of order $10L$ (where L is the spacial integral scale) on a side, and probably the particles should be about $\frac{1}{2}L$ apart on the average so that their statistics will be independent. Hence, approximately 10^3 particles must be tracked (we also need this number for stable statistics), with images taken at spacing of the order of the Kolmogorov time scale η/ν over a period of twice the Lagrangian integral time scale. This means the number of images is of order $2R_L^2/2$, and this should probably be at least 10^2 to be interesting. Hence, something like 10^5 particle images must be located in three dimensions. The computer processing of this data is also not completely trivial; among other things, the computer must identify which among the successive images corresponds to the same particle, which probably requires holding three holographic images in memory at once. Note also that to get a high enough Reynolds number the grid size will be large, and hence the tank will be large. We are talking about an expensive experiment.

We may also consider the possibilities offered by computer controlled experiments, automated data taking, and the like, as well as color graphics work stations and the flexibility and innovation they bring to data analysis. We have just completed exploration of an air jet and helium jet, using a shuttle mounted hot wire and Way-Libby probe. It is absolutely essential to have the data gathering computer-controlled, since the data are gathered by 1000 runs of the shuttle on each of a series of vertical cuts, and must then be faired, interpolated, and reassembled into horizontal cuts.

So far as color graphics work stations are concerned, we could mention the analysis by Godfrey Mungal of a rocket exhaust. A sequence of equally spaced photographs were taken of the exhaust jet; these were digitized, and electronically stacked on top of each other, and the stack turned 90° so that the edges of the picture were visible, whereupon it was evident that coherent structures were proceeding up the jet, more or less keeping their identity as they went. Of course, the information obtained is no greater (or perhaps less) than could be obtained by a statistical analysis of the pictures, or even of the turbulent/non-turbulent

interface alone (as carried out by Bill Schwartz a number of years ago) and it is not clear that it uses less computer time, but on the other hand it presents the results in a form that can be understood, and will be believed, by anyone. It is, of course, absolutely dependent on the capabilities of a color graphics work station.

There are a number of techniques that are being exploited at the present time, laser sheet illumination with smoke or bubble seeding, laser induced fluorescence with sheet illumination, and so forth. This makes possible visualization that is much more precise and controllable than has ever been possible before, producing often beautiful pictures. I am disappointed, however, that often nothing quantitative is done with these data. We are turning into a community of naturalists, content to describe the behavior of the latest wild vortex observed in its native habitat, when we could be extracting from these images quantitative data that could shed more light on what is going on.

Footnote:

1. Two quotes provided by C. R. Smith:

"Science walks forward on two feet, namely theory and experiment. Sometimes it is one foot which is put forward first, sometimes the other, but continuous progress is only made by the use of both --- by theorizing and testing, or by finding new relations in the process of experimenting and then bringing the theoretical foot up and pushing it on beyond, and so on in unending alternations."

Robert Millikan - Nobel Laureate in Physics, 1924

"[Rutherford claimed that he] liked to discover facts the reliable way, through experiments, without a lot of theoretical pettifogging. The experiments themselves should be quick, simple, and performed on equipment scavenged from the basement of the laboratory. Each test should built on the one before. The object was to find good, solid facts----did X happen or not? ---to find them first, and to let other people clean up decimal places. "There is always someone, somewhere, without ideas of his own, who will measure that accurately."

Lord Rutherford

Tony Perry (University of Melbourne):

It has been said that turbulence is the singular ~~unsolved~~ problem in classical physics. The application of the ~~knowledge~~ of turbulence covers a broad field ranging from astrophysics, meteorology, oceanography, chemical mixing and stirring, aero- and hydrodynamic drag prediction of ~~vehicles and transporters, combustion, etc.~~

For better or for worse, most people who have worked in turbulence have been engineers. Actually, to be more precise, I should say engineering scientists. There has been a great pressure on these researchers to produce short term results of immediate practical importance and so a lot of activity has been devoted to the tuning of constants in closure hypothesis schemes for engineering design applications but this does little in giving us an understanding of the turbulence process. Not enough effort in the past has been devoted to, ~~nor has there been sufficient funding for the study of turbulence for its own sake, for the purpose of gaining an understanding.~~ I think much more effort is needed in this direction and this is what I wish to address. We should be behaving more like scientists than engineers.

The research worker in turbulence has many tools at his disposal and the experimental approach is ~~only one approach.~~ For reasons which will become apparent in late talks (particularly by my colleague, Brian Cantwell), it will become imperative in the future for an effective research worker in turbulence to have his feet in many camps. He should be a good CFDer (Computational Fluid Dynamicist), EFDer (Experimental Fluid Dynamicist), and AFDer (Analytical Fluid Dynamicist). He should become involved also in computer aided analysis. And let us not forget good old fashioned physical thinking, dimensional analysis, and similarly analysis of data.

The most important tool, of course, is not the wind tunnel, nor the computer, not the measuring instruments, but the human brain! I have here a picture of the human brain. (See Figure 1). It does all the thinking and actually that's where resides the ultimate understanding. I want the human brain to represent our understanding of turbulence. The human brain, or understanding, actually stands on two legs. These legs help it to progress forward. The two legs are ~~experiment and theory.~~ (See Figure 2)

When I was doing my Ph.D., there was a third leg which sort of acted as a stabilizer, and that was the sliderule. Well, of course, these days we have what some people believe to be three legs, the third leg being CFD, as shown in Figure 3. But I think from a research viewpoint, this picture is not quite right. I don't think CFD is a stand alone activity, unless it's for the purpose of engineering design, and so on. In research it's more or less concerned with theory and experiment. A more faithful picture, I think, is as shown in Figure 4, and the computer is actually the muscles in these legs of experiment and theory. Experimentalists use computers just as much as theoreticians. A well balanced and productive human brain needs to feed on information from a variety of sources and a human brain needs to feed on information from a variety of sources and a good worker sees all of these as one and should be able to switch his thinking rapidly from experiment, computation and analysis.

Experiment is, of course, the most important. The final arbiter of any result will of course be experiment. The medium by which we discover new phenomena will, to a large extent, be by experiment. If a discovery is made by any other means, experiment at least will be required for the final confirmation.

What, then, are the central issues of turbulence research and what directions should experiments take? The only way I can answer this question is from a personal viewpoint. We need to learn how to describe the turbulence before we can understand it. So far, we have failed. We don't even have a definition for turbulence, although it is generally agreed that it has the following properties:

- i) It is unsteady.
- ii) It is three-dimensional
- iii) It is apparently random.
- iv) It is dissipative (i.e. viscous dissipation is converting kinetic energy of the motions into heat).
- v) The most difficult property of all, which causes trouble in both describing it and computing it is that it consists of motions which are spread over a range with nonlinear interactions among the scales.

Figure 5 shows some spectra of wall turbulence. The wave number range of these spectra increases as we increase the Reynolds number. The curves shown are deduced from recent similarity laws which have been proposed based on laboratory data (see Perry & Li 1990 J. Fluid Mech. 218, 405-438). From the figure two power laws are shown. One is a -1 law for low wave numbers and there is a $-5/3$ power law for the high wave number region, which is the so called classical Kolmogoroff internal subrange. At the highest wave number we have the dissipating motions. In the figure we see the size of the "window" which can be handled by direct numerical simulation (DNS) on a Cray computer. This is compared with the window for typical laboratory data. One can see this much larger than the DNS window but it is not that much larger. It will also be noticed that the -1 power law and the $-5/3$ are not convincing unless we go to a window almost equal to the size of the diagram and this would correspond to meteorological data or data carried out in a very high Reynolds number facility which has yet to be built.

One of the most important regions is the dissipation region. We need an understanding of this region if we are to have any hope with subgrid scale modeling.

What do these fine scale motions look like? Spaghetti? Lasagna? Macaroni? Noodles? Fettucini? Vermicelli? Ravioli? Is it isotropic? Is there an energy cascade and how does it work? Is there an internal subrange? Is there really a $-5/3$ power law or is it just a rumor? What do the large scale motions look like? How are we to describe them?

What we need to do is go higher and higher Reynolds numbers to verify these similarity laws which have been deduced from a very small window of Reynolds numbers. DNS is certainly not going to help here but it will be helpful in areas I will mention later.

We need to go to high Reynolds numbers but not increasing U or by decreasing the kinematic viscosity, but by increasing our length scale. That is, we should go big and relatively slow so as to avoid spatial resolution and frequency resolution problems. We should continue classical measurements (e.g. spectra, correlation, and broad-band turbulence intensity measurements). It is quite obvious that the hot-wire anemometer has clearly proved its worth in this activity. The laser doppler velocimeter has been singularly disappointing and

there are many reasons for this:

- i) Seeding problems.
- ii) Low data rates.
- iii) Randomly sampled signals and
- iv) Sample biasing problems.

However, another area we need to work in besides these classical measurement is flow pattern topology and geometry.

We need to know and understand the topological structure of three-dimensional steady and unsteady eddy motions (including separation patterns) which is done with the aid of critical point theory. We also need to apply this to the fine scale dissipative motions. The most basic fluid motion is the motion as seen by a non-rotating observer who rides with a fluid particle and looks at the universe which surrounds him. This is given to first order by

$$\dot{x}_i = A_{ij}x_j$$

where $A_{ij} = (\partial x_i / \partial x_j)$

which is the velocity derivative tensor. Also

$$A_{ij} = S_{ij} + R_{ij}$$

where S_{ij} is the rate of strain tensor (symmetric) and R_{ij} is the rate of rotation tensor (skew symmetric).

Some promising developments have recently occurred at a CTR meeting at Stanford this year where the local topological structure of the dissipative motions generated by DNS were studied. The tensor, A_{ij} has three invariants, P, Q and R which were mapped out for highly dissipative motions (these are analogous to the I, II, and III invariants of Lumley). Brian Cantwell might elaborate on this later. The real challenge to experimentalist is to measure accurately the instantaneous values of A_{ij} in an entire field. Optical methods no doubt will be needed. Particle tracking and speckle anemometry have been very successful for particles which lie in a plane. But what about 3-D? and what about at high Reynolds numbers? Of course we need to know the geometry and topology of many other quantities besides the velocity field, namely the vorticity field and the pressure gradient field. These latter quantities, we will be confined to low Reynolds numbers.

By and large, I feel very excited about future experimental work. Mind you I am an optimist. I want to now give you my impression of what seems to be the problem in this country regarding experiments. I am an outsider and I don't know all the detailed problems you have. I have work here periodically for short periods, but I still consider myself an outsider. Perhaps, as an outsider, I might be able to offer a fresh viewpoint. The problem as I see it is that experimentalist feel like second class citizens by the way they are funded. Also, it is very difficult to attract young people into experimental work. They all want to be CFDer. Let's look at why.

Firstly, an EFDer has to be highly skilled in designing his experiment and instrumentation. He has to have knowledge of electronics, optics, photography, mechanical engineering, systems theory, data acquisition systems, computer systems, computer software development, and signal processing. Finally (I almost forgot) he should also have a knowledge of the

theory of fluid mechanics. He has to spend a great deal of time talking to instrumentation and computer salesmen. He has to be an expert in telephonology (i.e. how to get information on the telephone). Most of all, he has to know how to talk to machine shop foremen, machinists, and technicians. He has to live in grimy and noisy conditions underneath a wind tunnel and has to be able to use a screwdriver, monkey wrench, drill press, milling machine, lathe, or soldering iron at short notice during emergencies. Finally, once he has built and tested all of his equipment and debugged all systems, he has to gather data and examine it and cross check it for accuracy and analyze it and display it with computer graphics. If the results are surprising and unexpected, he might have a breakthrough. If the results are rather boring and as one would have expected, he might be tempted to change the aim of his experiment and start over again, (five years down the drain) or offer his data up to the CFD modelers for confirming their codes, which is almost like defeat.

When he has achieved all of this and has obtained his Ph.D., he then has to be content to live in poverty on soft money for at least ten years before he has even half a hope of getting a long term, three year, non-tenured position.

Let's look at the career path of a CFDer.

He works in a nice clean air conditioned room (no noisy tunnels for him). He is able to wear neat clothes and squeaky clean shoes and can get results for his Ph.D. simply by having only a couple of the skills required by the experimentalist. He is immediately given a faculty tenure track position, drives a Ferrari, and very soon joins a country club.

The simple solution to the problem is to raise the salary scale for experimentalist (say to about three times that of a CFDer) and encourage EFDers to do their own CFDing. They should be doing this anyway because they are a more skillful sort of person. This would immediately remove the first class/second class syndrome.

Well, I have given you the broad solution. I will leave it up to you to work out the details.

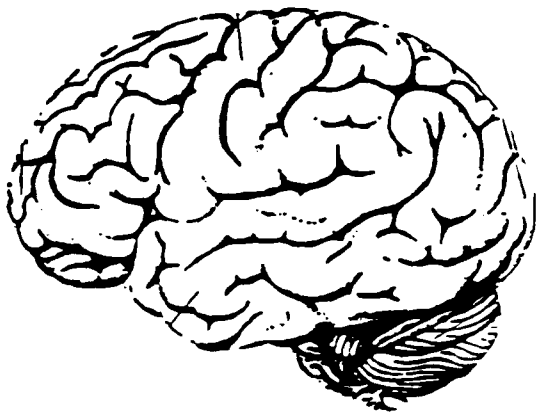


Figure 1

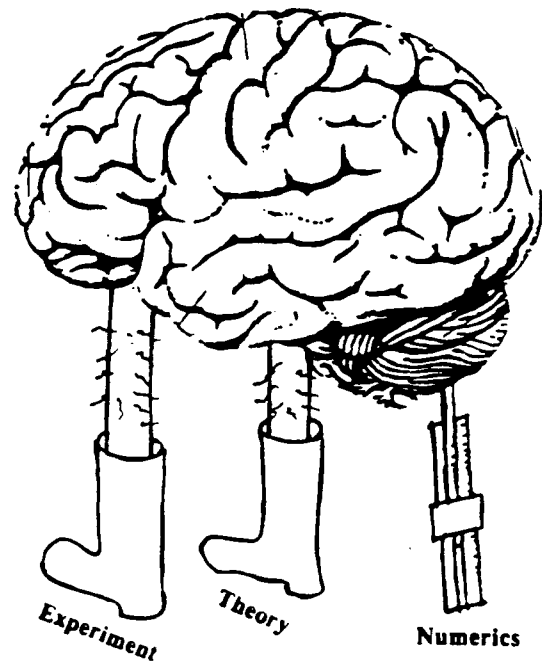


Figure 2

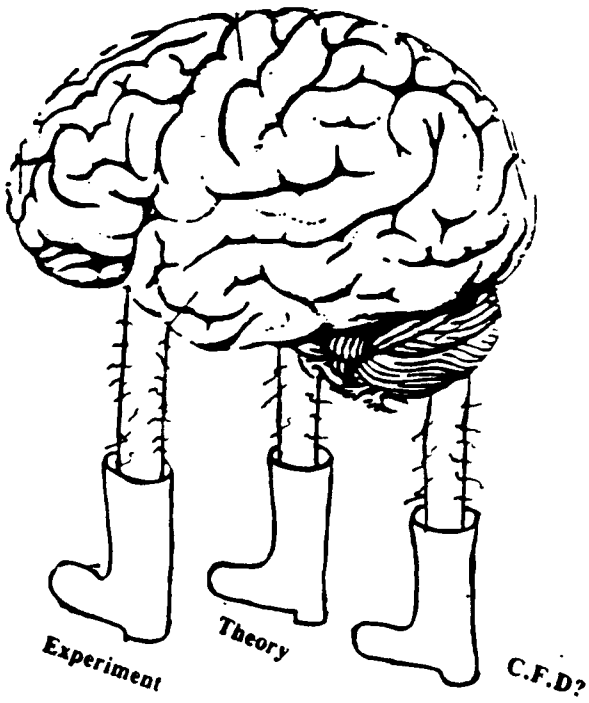


Figure 3

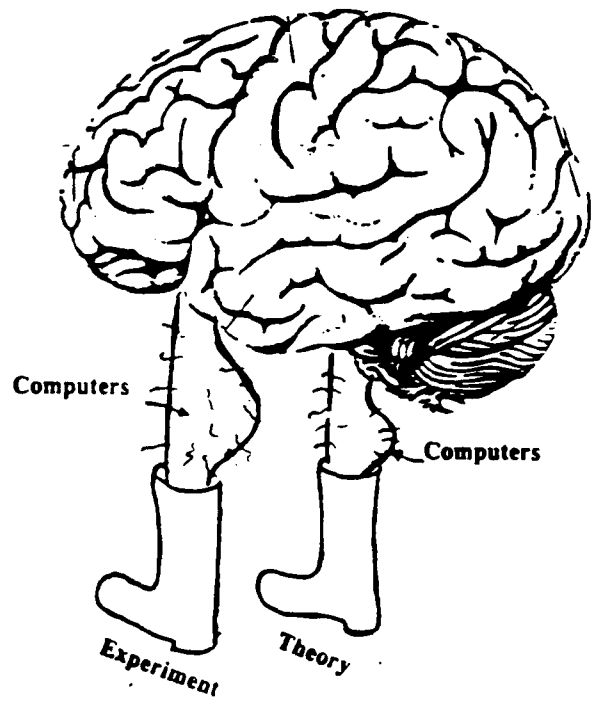


Figure 4

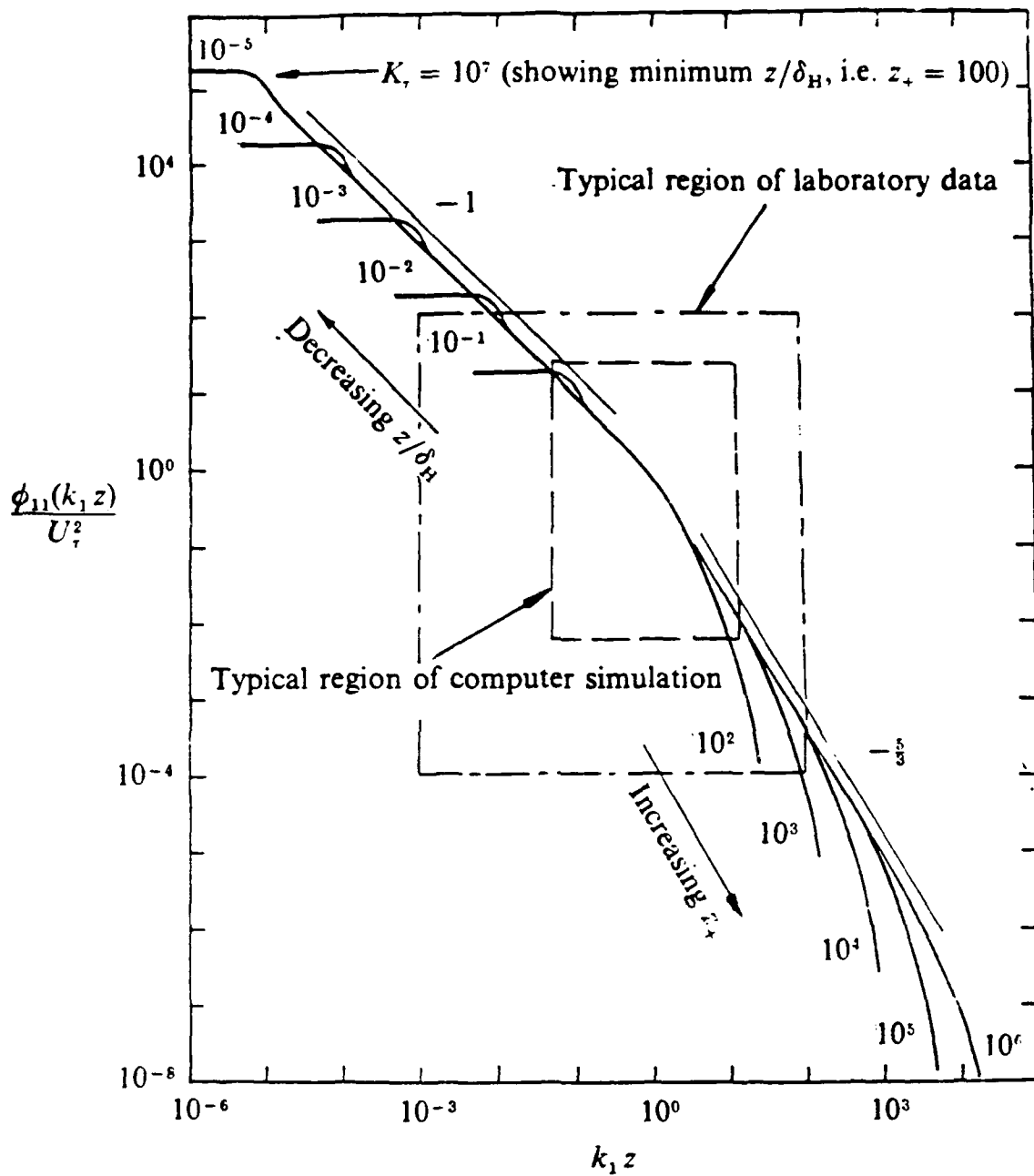


Figure 5

Power spectral density of the streamwise velocity fluctuations in the logarithm law of the wall region of a turbulent boundary layer conjectured from recent similarity proposals. Here z = distance from wall, k_1 is the streamwise wave number, δ_H is the boundary layer thickness.

Discussion:

The discussion opened with a number of position statements attempting to identify the fundamental problem(s) facing the turbulence research community. National laboratories, academia, and industry positions were all represented. This opening phase of the discussion highlighted the lack of communication between the various components of the community.

Dennis Bushnell: John (Lumley), I think it was, said that as far as fundamental work in this country, we are not in good shape. I'd like to indicate that, in the 19th century, in the U.S., we had an excellent patent literature and were fairly poor in the fundamentals. In the 20th century, in the U.S., the patent literature is declining. We're fairly good in fundamentals, in that we are, I think, close to the top in terms of Nobel prizes. However, there is an inverse relationship between GNP growth and Nobel prize awards. So, in other words, we do, in fact, have excellence in fundamentals, to our detriment, evidently in terms of GNP growth. Since the 50's, the engineering schools have been producing engineering scientists; we have lost or are losing our technological excellence. The current emphasis on applications is an attempt to do something to aid the U.S.'s competitive stance by several agencies (e.g. NSF, the Air Force, NASA, etc.). There is a problem, gentlemen.

This meeting is a small backwater, an eddy if you will, as a reaction to some of the attempts to fix that problem. I don't think it's a problem that we're not doing our fundamentals. The problem is we're not doing our applications. Thank you, gentlemen.

Fred Browand: Do you have a response, John?

John Lumley: I said what I had to say. I don't see how I can respond, except to say that I disagree.

Wyganski: Historically, turbulence research was done at three levels:

One, in universities where most laboratory facilities are serving one principal investigator and three or four research assistants.

One, in government laboratories (like NACA or NBS used to be) where some most significant turbulence research was done.

One, in industrial laboratories, a lot of which were associated with the aeronautical industry.

What happened to the government and industrial labs in which research in fluid dynamics was done? Fluid mechanics group at NBS became part of chemical technology and shrunk to a non-existent level. NASA, with the space program being its prime mission had reduced the research in turbulence and redirected it to become mission oriented. The industrial research laboratories at Boeing and Lockheed were closed entirely as a result of economic austerity plans, while the one at McDonnell-Douglas is heavily burdened by industrial type of

developmental work. The virtual disappearance of research activity in industrial and governmental laboratories snapped an important link basic and applied research in fluid dynamics. Therefore, one community, the university community meets occasionally and discusses how to proceed with their research (Research that germinates at a university and often ends there as well). The other community, the industry community does not communicate with the university community and vice versa. I just attended an AIAA Applied Aero Meeting and heard a few talks about turbulence that shocked me. I had a feeling that I am attending a meeting of a different era, that it takes place in the 30s. I believe that the interaction among university, government and industrial labs is vital and this interaction should be reinforced. Perhaps turbulence research at NBS should be revived, it could be called a 'turbulence laboratory for complex flows' or 'applied turbulence laboratory', in order to provide the necessary link between industry and university.

Steve Kline: Let me go back to what Dennis Bushnell said. I agree with him. I want to say why, and give some references to more complete discussions. This group is talking of turbulence research and why it ought to be supported. Before I further, let me remind you that I have been doing turbulence research for more than 30 years. Obviously, I think it is important too. However, we have misunderstood the relations between scientific research, such as that on turbulence, on the one hand and innovation, economic competitiveness and the welfare of our national economy on the other. We have all had in our heads a model of how this works something like the following: one, you do scientific research, then you do development, then production, then marketing. I call this the linear model. The linear model is a very misguided, simplistic model; while is not totally wrong, on balance, it suggests more wrong than right actions with regard to innovation and competitiveness. Is there a better model? Yes, there is and here I have to apologize because it is my model. I refer to it because there is at present no other equivalent. I call the new model the chain-linked model; I first recorded it in 1985 by synthesizing experiences in a number of industries. The model has been somewhat improved since then, but not changed in major ways. The chain-linked model is now used nearly everywhere in Japan, by some US federal agencies, and is being built into the new data base the European OECD is preparing.

Why has the rapid acceptance of the chain-linked occurred? As far as I can tell there are two reasons: (i) experienced managers of R and D long had known the linear model was quite bad, but they had nothing better, and hence used the linear model as the only means for thinking about innovation; one cannot, after all, think about nothing; (ii) the chain-linked model is a very big improvement; based on 5 years of feedback, the chain-linked model appears not only complete in the sense that all processes which occur seem to be included, but also the model has surprisingly good qualitative predictive powers; neither of these things are true in the linear model.

What does the chain-linked model say about the relationship of research to innovation? It says several things:

- (i) Research does not exist just at the entry gate to innovation; very important research occurs concerning production processes;
- (ii) The most important researches commercially are those usually thrown back from the work because these are the ones that overcome the current roadblocks; we can call such research technology-induced-science. Notice that technology-induced-science arises from a flow of information opposite to that implied by the linear model. Moreover, it is often untrue that purer is better in research; the very best research has often been suggested by real world problems. After all Prandtl was motivated by the needs of wing theory when he set

down what we now agree is the genesis of modern fluid mechanics in his boundary layer theory, and he did this one year after the Wright Brothers demonstrated powered flight.

(iii) We do not design from this years' research alone any more than we live only in houses we built this year. We design from the totality of available technological and scientific knowledge; I put technological first because on balance it is more important in designs that the scientific knowledge although often both are critical. If you think that technological knowledge is unimportant, you might ask yourself how we design combustion spaces?

These are only three of the reasons why the linear model is inadequate and the chain-linked model is far better. All together there are many reasons; five of them are each quite important. A more complete story on these matters is now available in two different versions as Stanford ME Reports INN-3, and INN-4. INN-3 sets the problem in very wide historic and cross nations contexts and discusses in some detail what the Japanese have done and are doing in comparison with the US. INN-4 examines the policy implications for management of R and D at the corporate and at the government level which spring from the linear and chain-linked models respectively; the two sets are quite different. INN-3 will appear as three articles in Chemtech in 1991. INN-4 is now in print as part of the proceedings of the First International Conference on Science and Technology Policy held in Shimoda City, Japan in February 1990. However, I will be glad to send either or both reports to anyone in this audience who asks.

From a slightly different view, when we examine very complex systems such as those we normally encounter in manufacturing an airplane, a computer or an automobile, we find we have no theories for the complete systems, only for some components. This follows from examining a standard index for the complexity of systems, C. C for turbulence is 4. C for a single human brain cell is more than a billion. Thus for systems like an aircraft manufacturing company C is more than 10. As a result, we can improve such systems only by open loop feedback, that is, look at the system, ask how can we do it better, try the change, and keep iterating. (I will be glad to supply the details about the complexity index to anyone in this audience also; ask for Rept. INN-5). The Japanese call this mode of research "kaizen"; some economists call it learning-by-doing, but the name is not the important point; it is rather that this is a critical necessary fir if research which we do not ordinarily call "science." It is precisely this form of research that the Japanese have used in creating what is now being called "lean manufacturing", and it is lean manufacturing that has given the Japanese their major edge since the time it first came together in Toyota in the early 1970's. Why do I digress into manufacturing? Because manufacturing, not science, is the core of economic competitiveness.

This key role of manufacturing in economics is not a new situation. In the 19th century European economists kept coming to the US and remarking, "We don't understand this. These Americans are a bunch of barbarians. They have no culture and they do no science, and yet they are outproducing us by a wide margin." This was indeed true. Science in the 19th century was essentially all in Europe. There were only 3 US scientists of significant note through the whole 19th century: Joseph Henry, Benjamin Thompson (Rumford) and J.W. Gibbs. No one in the US understood Gibbs justly famous 3rd paper when he published in the 1870's thereby creating ad novo the entire theory of chemical equilibrium. However, leading workers in France and Germany understood the implications for chemical industries clearly, and they therefore personally translated Gibbs' work almost immediately. More generally, it has been true throughout the entire industrial age (since about 1776) that any large nation which achieved a significantly leading system of manufacturers quickly became an economic

superpower worldwide, and conversely throughout this periods there have been no world economic superpowers except for those nations with at least near competitive systems of manufacture (except a few oil-rich nations). One cannot say the same about leading science.

Thus we believe that the linear model at our own economic peril. As Dennis Bushnell said, the correlation between high science and growth rate of gross domestic product of nations is negative (for details see INN-3 cited above). This suggests we do not do high science and then get rich, but rather the other way around.

What does all this mean for scientific research and for that branch of it we know as turbulence? It means that we have for a long time thought in terms of 'linear' model that misleads us, seriously. I was raised on this linear model; we all were. We all have in our heads many ideas therefore that are wrong about the relation of science to innovation, and we need to rethink them and to reset priorities based on this rethinking. Does this mean should do no turbulence research or should not ask for funding? Of course not! It does mean we need to look again at three things: (i) what we plan to do with the results of our science research; (ii) at the priorities over the total area of technology including the related science; (iii) what we teach our engineering students about what is important in the real and acutely competitive world of the 1990's and beyond. It is these matters and these questions which underlie the need for the discussion we are having about priorities and funding for turbulence research.

Val Kibens: I'd like to introduce the industrial perspective, here, as a participant, and somebody who really has to look at this as something to which I really need answers, in terms of planning just exactly what do we do next year. What do I do? How do I develop a research program? What do I do with the change in the situation?

I like very much what Wyggy said, that there are two approaches to the strategy of doing turbulence research. You can attempt understand it, or at least you can attempt to try to control it. We have tried to do the latter. Understanding certainly is something I come back to in looking for guidance. But, we have tried to control turbulence. That's where the money comes from in our shop. So, what we try to do is to work through problem statements.

I thought it would be very appropriate to look at this overall objective as I would present it to our management. This is how we present a problem that we will address through research. We want to develop something that will control something that will make the airplane fly better, that will make it be quieter, that will impact the bottom line. And I have to do this by referring to what I hear at the APS meeting, what I hear at workshops. Then I have to go back and justify specific things that I will do to my management for which they will give me funding.

Back to the record. Currently we are making major investments in various parameters. Our answer has been to essentially instinctively focus on near term support for airplane problems. That is the unspoken strategy at MDRL right now. Do something so you will be useful, so you will be around to fight another battle. It's short term strategy. I think it has to be underpinned by a longer term strategy. So all I really wanted to bring up was that issue, and the question, "what would you do?" Did the specific issues that involve, not only our research lab, but also the entire industrial research base. It's a problem that we need to address.

Bill George: I'd like to follow up on John Lumley's comment, and more directly on Dennis

Bushnell's comment. I think the analysis you present is much too simplistic. This is probably not the place to argue the details. For example, one might find it more revealing to plot the number of patents versus the number of MBA's over those same periods, or patents versus defence spending. In my own experience, the problem we have directly in fluid mechanics in turbulence is that industry has failed to effectively utilize our product.

Let me offer an example. In the late 60's, I played some small role in the development of laser anemometry. That instrument was ready for industrial application in the mid 60's, and was heavily utilized in Europe. Most of the sales of commercial vendors who were making Laser Doppler Anemometers were, in fact, to European or Japanese industry. It's only very recently that the sales have picked up in the United States, a decade later - and even then mostly in Defence. Now what happened? Industry would hire our students, not just mine, but other peoples', and literally set them there with no equipment to work on or nothing to do. Now why is that? They were simply unwilling to make the capital and research investment in the future.

Let me suggest where the problems might be. I suggest that, for example, the problem that the federal government, and NSF in particular, is trying to fix is a problem that industry itself has created. (I feel that they also blame us for their mess.) We have a national policy that siphons our best talent off into the defense industries. We are focussed on short term planning that does not make the kind of commitment necessary to build research capability and translate ideas into practice. I've often thought perhaps that the best thing we can do to affect American competitiveness is to make computer screens wider so that people can look at a ten year spread sheet instead of just three years at a time. Maybe the problem will solve itself when the Japanese buy up all of our companies. Japanese management seems to do a marvelous job of long term planning; and they seem to be pretty innovative, especially at implementing our ideas!. I'm not sure what the solution is. But, I don't buy the argument you make, Dennis, that the problem is that we're focussing on fundamentals. I will argue, tomorrow, at some length, that I think our education system is in reasonably good shape. It's the people who are defining the policy in the country who are screwing up, both in industry and in government!

Bushnell: Let me rebut that a little bit. I have a little personal experience. For the last fourteen years, we've had an offer out to the university community that we would fund any non-ridiculous new idea, that you couldn't prove on the back of an envelope that it would violate the second law, which would reduce turbulent skin friction coefficient or control turbulence to diminish it in wall boundary layers. This has been an open invitation to the university community. When I talk to that community, they say, "no, no, we don't want to think about that problem. That's much too applied. We want to understand what's going on and do the fundamentals." There is a singular lack of interest in the university community in doing something that applied and in generating ideas.

Rich Wlezian: Until recently I was at McDonnell Douglas, so I still retain some of the industrial bias. I want to add a different viewpoint to the things that Bill George said. That is, he brought up the issue that industry is not applying advanced experimental techniques. I agree with that to a certain extent. However, I don't think it's appropriate to infer that industry is somehow deficient for not using these techniques. Instead, I think it's our role to do a better job of introducing advanced techniques to those doing applied research. These techniques would be more widely implemented if we did a better job of conveying the information to the applied community. At MDRL, there is continuous technology transfer between the fundamental research community and some very applied programs. As a community, however, I don't think we're doing a very good job of reaching out to these

programs. I think we tend to present these techniques and expect someone to implement them; when that doesn't happen, we tend to criticize those doing applied research. There is a large pool of users out there who would be willing and financially able to support some of the instrumentation development if we simply did a better job of communicating with them.

Browand: That comes back to Lex's point this morning, if turbulence research is so important in all of these various technological areas, why aren't turbulence researchers more visible? We still haven't dealt with that.

The discussion continued with additional statements from Profs. Bert Hesselink and Anatol Roshko. Hesselink requested a formulation of focussed problem statements with which the turbulence community could approach the various funding agencies. Roshko pointed out that fundamental research has already made significant contributions to applied engineering, and that there has been little credit given for those contributions. Kline then introduced the term, 'technology induced science', which, he argued, should be the driving motivation for all future turbulence research.

The discussion next turned to the relative merits of a large national experimental facility.

Browand: Dennis, you're on this list of open mike topics.

Bushnell: This concerns something that Wyggy said about national labs. The IIT tunnel, sponsored by AFOSR, was a decision made several years ago to put a large, high Reynolds number facility in a university specifically for outside users. We've recently done some Mach 6 mixing tests at Langley by CalTech and VPI, which is going to result in at least a Ph.D. dissertation, where the students were on site with some of the faculty for up to three months each. That experience has worked quite well, and I'm sure many other people have had similar interactions. There is a central U.S. combustion laboratory with extensive diagnostics available in this country. The cost of emerging diagnostics and large scale facilities, I think, draws us toward the national lab concept. And of course the high energy physics people have been into the national lab route for many, many years. The business of trying to replicate \$4-500K laser lab equipment for diagnostics at universities is just not in the cards. It's not going to happen. Between that and a large high Reynolds number facility, let me assure you, that at the national laboratories, where the large scale facilities are available, we are very open to universities coming in and wanting to use those facilities for fundamental studies. We have no problem with that. Thank you.

At this point, there were a number of points of clarification regarding Bushnell's statement. The primary issue was the need for a cohesive policy for the use of such a facility. An opposing point of view was put forward by Prof Gary Settles.

Settles: I'd like to point out what I think is the downside to this national facility concept. That is, we have an advantage in turbulence research, in that we don't have to use a big expensive national or international facility like a linear accelerator. We can have our own fairly small scale facilities, university scale facilities, where we can train students and actually accomplish something useful. If you go to the kind of scenario where you only have big national facilities that cost a lot of money, you schedule several years ahead of time. You schedule for a month of time where you go in and do your preprogrammed testing; but that's not research. You also run into the difficulty that you have union labor running the facility and it costs a fortune. You also have restrictions, for example, on whether you can use lasers or not. I've tried to do research in big facilities, and I've seen it. I say we have to keep the small investigator with the small facility. That's an important part of this whole

arena of research. If we lose that, we'll lose something very valuable.

Bushnell: You're right. There's no intention that we should lose that. The issue is to expand the parameter range, to go to add points and to go to large scale, as was suggested by the original two of the three speakers. O.K. This is an option which is open to us.

Lex Smits: That's exactly the focus, though. I think we really need to look at those questions that are driving the field. If we decide that, for example, we desperately need information at Mach 10, then this is where we need to spend some effort, put some money, get into it, and do it right. We can probably only do that in a national facility. Of course we all like the idea of our own wind tunnel where we can go into the back room and we can control our project completely and totally, and work it out ourselves without having to deal with other people, and make arrangements, and so on. But that will only get us a little bit of that parameter space, the hole in the center of your chart. To expand that parameter space, we will need to obviously talk about facilities that are very specialized and may not be one investigator facilities.

But, the important point is "What is the problem you're trying to solve?" There's no point in building a facility unless it's solving a problem for us.

There was a brief return to the discussion of the role of fundamental research in technology, and the different ways of viewing academic research programs in the context of advancing technology. This was then followed by a discussion of the need for advocacy by the turbulence research community. This idea was introduced by Dr. Jim McMichael of AFOSR.

McMichael: It seems to me that it's not just the job of the speakers that we heard this morning, to identify the problems that need attention. It's really the job of the community as a whole. The question of "what problems ought to be done?" is really one of the issues involved in the overall advocacy of fluid mechanics and turbulence research. The question really is, "what are the advocacy arguments? and where do you make those arguments?" And more importantly, "do we have a mechanism within the community to answer those two very basic questions?" I suggest that, right now, we do not.

Don Rockwell and Fazle Hussain returned to the issue of defining specific turbulence research problems. Rockwell pointed out the need to package these problems in scientific, application, and layman's terms in order to ensure continued financial support. He also stated his desire to see discussion of specific technical questions and future instrumentation directions at some point in the meeting. Hussain stressed the importance of measuring the vorticity field as a way of understanding coherent structures, and he also supported the opinion that certain experiments must be done in large national centers:

Hussain: I would like to comment on critical experimental needs in turbulence. Perhaps one of the more interesting facets of turbulence is vorticity dynamics, and I think one of the greatest challenges in turbulence is vorticity measurement. Of course, we cannot measure true vorticity in laboratory turbulent flows. It is unlikely that vorticity can be measured in the foreseeable future, because vorticity is a point-wise property and is characterized by extremely small length and time scales which are not easy to resolve. You can measure with some accuracy micro-circulations in different planes in a flow; this is unavoidably an area average.

I think vorticity is critical to understanding turbulence. Even though many agree that

coherent structures (CS) are very important for controlling turbulence phenomena such as transport of heat, mass and momentum, chemical reactions and combustion, and management of drag and aerodynamic noise, unfortunately, the overwhelming majority of CS studies have involved only qualitative descriptions primarily based on flow visualization, which is often not only misleading but provides incorrect information. We have made this point many times. I think it is quite useful to view CS as vortical entities and interpret CS behavior via the more mathematically tractable tool of vortex dynamics; of course, vortex dynamics and, in particular, 'local induction approximation' has its own limitations. I think it is only through vortex dynamics that we can get a handle on the dynamics of CS. Thus we should not only focus on accurate measurement of local vorticity, but also on the definition of a vortex in turbulent flow. The latter is a very controversial subject, with no clear answer in sight. 'Kinematic vorticity number' defined by Truesdell is useful in this regard, but does not always work. As far as I am concerned, this definition is essential for quantitative discussion of evolution and dynamics of CS. Unlike helicity, which is not Galilean invariant, I suggest, 'complex helical wave decomposition' for an objective definition of CS. This decomposition provides a frame-independent definition which may be the most appropriate way to study CS.

John (Lumley) definitely made a significant dent, but I think there needs to be a considerable amount of additional effort in trying to link the fairly rich field of nonlinear dynamics and spatio-temporal chaos with turbulent shear flows. One of the areas which I think is rather interesting and has many implications in mixing and chemical reaction is the reconnection of vorticity, the so-called vortex reconnection or cut-and-connect, which is an example of practical application, and is of profound contemporary interest, of non-preserving topology; also associated with it is the question of finite-time singularity of the Navier-Stokes equation in the limiting case of zero viscosity. While theorists seem to be highly concerned about it, we do not need to worry too much about it.

Perhaps tomorrow Fred (Browand) will focus on what I will say next. I think that future needs in turbulence research must involve measurements in 4-dimensional space. We need instantaneous information about turbulence structure, in space and time. Two methods in which we have some interest are holographic particle velocimetry and electron spin resonance velocimetry. The latter may indeed be a very powerful tool for turbulent flows.

While we're at it, I want to put in my two pennies worth on critical experimental needs in the future. Here I will mention some examples from what I proposed some years back: Uriel Frisch and I have discussed this subject a number of times, and I have had subsequent discussions with Steve Orszag and Bob Kraichnan. I think there is indeed a need for national centers for research in turbulence like in basic physics. Unfortunately, the community here as well as the community at large seems to be very unwilling to claim that turbulence is as much a fundamental problem as high-energy physics is. That may be one way of trying to avoid having to rejustify our existence every year. Why can't turbulence be supported as an area of fundamental inquiry? I think some national or large-scale centers are unavoidable, so that large, unique facilities can be shared by all qualified researchers. One example of specific experiments that I have proposed a few years back is a pipe, of the order of 5m in diameter, 5km long. The dimensions are not necessarily precise, but I have a reason to propose such a facility: Boggy I think once mentioned in a meeting, I don't know if he's in the audience now: "How come you always run out of flat plate when things just begin to get interesting? So, this is an example where we may beat this problem of running short of length of flow. I think you can agree that such a long pipe, if fed with turbulence from smaller pipes, will be fully developed before its exit. Definitely the major trouble with wind tunnels is longitudinal and transverse acoustic modes which most experimentalists don't even want to talk about, don't want to face, and in the large pipe I proposed you would not have the

residual effect of screens, contraction, freestream turbulence, blade wake, corner eddies, end wall turbulence, and so on.

So one possibility perhaps is the so-called World Center of Turbulence or National Center for Turbulence Research like NCAR. I do think that such large-scale facilities are necessary. We must be willing to think big, and I feel that turbulence research is always trying to justify how it will help technology and we immediately get into the trap of having to rejustify every year; this way, we dissipate a lot of our energy. Therefore I think we are our worst enemy. I don't see why we can't push turbulence as a basic problem in physics.

The last problem, with reference to Tony's (Perry) question regarding how to attract students to experimental turbulence. I suggest we offer each student willing to do experimental work a Ferrari when they join the research program.

The remainder of the discussion focussed on the advocacy issue.

Wyganski: It is true that we do expensive science quite a lot. It is true that there are some large scientific experiments that take a lot of money. Where are they? Well, the physics community manages to somehow excite us. You ask yourself, "why"? They may have a very complex theory in mind, but they come to the public with a very simple model. They convince us that our entire origin, that our entire existence depends on understanding that model. So somehow, as a result of that conviction, the public at large gets sufficiently excited that you hear people (laymen), lets say, discussing the merits of the Hubble telescope as it was about to be sent into space with great admiration. You hear it from people who have nothing to do with science. Somehow the physics community managed to excite the general public. They managed to make their models as topics of discussion at cocktail parties, or at any other social activity and that, I think, is the source of their success. As a result of that success, they built very expensive experiments funded by the public and with these experiments they educate the next generation of physicists. They eventually replace the model with another model and the process repeats itself. We never came up to the public and said: "Here is the model which, if properly explained, may have a tremendous impact on your lives. Help us to understand it". Then try to explain it to the public in the most simple form. We failed to let the general public understand what is turbulence all about. I think this is partly our failure towards the general population and towards the political system in particular.

Garry Brown: I'll just make a quick observation. I don't get very excited about distinctions between fundamental and applied. My own feeling is that the criterion is essentially unimportant. I agree with Wyggy that physics has no problem with large facilities because the question that they're posing can be seen by dispassionate people as being important. The question, in some sense is hard to define, but it's recognized as being important. I don't feel that turbulence is driven so well by simply a curiosity motivation as it is by technological motivation. I think, myself, if there's an actual program required, it's because it's being driven by a need to be able predict turbulent flows for technological ends, essentially. I think the cause for a national program depends, then, on whether we really believe we can make real gains in predictive capability. And there are two kinds of predictive capability that I think are important. One of them is the numerical predictive capability, where you try and get accurate answers to problems. Another, is where we simply have no idea; we don't have any numerical capability at present. What we would seek is some kind of predictive capability which is essentially more physically based, a simple model. Now, I think there is a

rationale for large scale national programs to achieve progress in both of those areas. I think, the key question is whether, at the present time, we see the possibility of progress being significantly greater than it may have been say a decade ago. Are there reasons why we could have much greater predictive capability, much greater progress in predicting flows than we might have had a decade ago? I think there are reasons. And I don't think we've quite focussed on what those reasons might be, and how they might then be used by an advocacy group to get wider recognition for our particular effort. And I think that's, perhaps, a part of the problem. People are concerned about the whole pie shrinking. It will only not shrink if there's really a pretty clear idea of what new predictive capability might be possible, why we believe we're in a good position to make a lot of progress in that in the next decade or so.

At this point, the idea of small working groups was raised to address particular issues already raised, and which might come up later in the workshop. Browand closed the discussion by presenting his thoughts and observations on meetings and journal publications. He pointed out, that as an editor of JFM, he has observed that the delays in getting finished results into print are a responsibility of the authors/researchers themselves as well as the responsibility of the review process. He concluded with a number of suggestions for optimizing the publication process. These included shortening the submissions, focussing on the fundamental questions addressed by the research, and reducing the turnaround time after the author receives the reviewers' comments.

SESSION 2

Computers and Experiment

Session Chairman: Anatol Roshko, California Institute of Technology

Session Recorder: Rabi Mehta, Stanford University

Brian Cantwell (Stanford University):

I think that some of the issues that have been raised, the comments about high energy physics, their ability to manipulate the federal government for funding and so forth, should be discussed. All those things are very important and I think that this is worth spending time thinking about, for us as a community. So let me just begin with a few remarks in this connection. It's probably not a bad idea to think about how they operate and the kinds of numbers they consider to be big money. I am reminded of a machine called ISABELLE, which was built at Brookhaven quite a few years ago. I guess it was in the mid to late 70's that ISABELLE was primarily constructed. After spending a significant fraction of one billion dollars on the construction, it became clear that ISABELLE was a failed machine which would never meet its design specifications. So the high energy physics community formed a panel of experts which studied the problem for one year and came back with, first of all, the recognition that ISABELLE had failed and should receive no further funding, but also with a strong recommendation that the reason it had failed was that it was too small and that what was required was really a machine at least ten times bigger. The panel recommended that the nation fund the construction of a large new machine. Moreover the panel also recommended that no other major new project should be funded. Out of this came the superconducting supercollider which was projected to cost 7 to 10 billion dollars. I would like to note that recently it has become apparent that the diameter of the passage which contains the accelerated particles will probably have to be increased requiring modification of the magnet specifications increasing the projected cost by an amount comparable to one Stanford endowment. So as far as high energy physics is concerned 1 billion dollars is a throwaway in the effort to advance the frontier. As we think about the future of fluid mechanics we should probably keep this in mind and not sell ourselves short when we consider what it will cost to make real advances in our knowledge.

Now I also think that we need to realize that the problems we are discussing are generic to science in this country and not just a problem faced by fluid mechanics. Yesterday (Sept 4, 1990) I picked up a copy of the New York Times and it has a lengthy article on how small-scale science is feeling the budget pinch. It is quite an interesting article, well worth reading, and as you read you'll see many of the things we're saying here today. The complainers are not turbulence researchers but, for the most part, biologists and people

working on condensed matter physics. I always thought they were well funded, but the fact is that they are also feeling the pinch. One of the most interesting quotes in the article is from Eric Block: "it is very important to set priorities so we preserve the infrastructure and look at the big project secondarily". That is not the message I got from Eric Block and the NSF in the recent past. In any case that is the policy he is prepared to state through the New York Times and I think everyone in this room would agree with it.

Others at this conference have noted that we need to make our work better known to the public at large. Again the physics community is mentioned for its adeptness at getting the public interested in and excited about physics research. Large facilities like the SSC become much easier to push when the public is behind them. Our research is not known to the public and I can illustrate this with a personal story which many of you will find familiar. Whenever I travel to the East on business I try to include a visit with my relatives who live on Long Island. Occasionally I am asked to explain (in non-technical terms) just what it is that I do. I explain that I work in turbulence, trying to understand the aerodynamic forces on airplanes, buildings, trucks and other objects. I try to explain how a hot wire works, some of the ideas behind streamlining and so forth. Although everyone is very polite, their eyes soon glaze over and the conversation moves uncomfortably on to another topic of more immediate interest. Then last spring, when I came back for the Cornell meeting on "Whither Turbulence", I brought a copy of the paper I gave there, and it included a few references to some of the work on Chaos. Included was a reference to Gleick's book which was a recent New York Times best-seller on the subject. I left the paper for my mother to read who, although not technically trained, spent her professional life as a reference librarian and is extremely knowledgeable on a broad range of subjects. Upon my return, she had read it and said, "Now I understand; you work in chaos!"

It's not just the high energy physicists but physicists in general who seem to be quite adept at getting their message out, getting the public excited about what they do and in turn influencing the funding of scientific research. There is nothing particularly wrong with that as long as it does not lead to a distorted set of national priorities. Should we approach our funding problems the way the physicists do? I'm not sure we should. I think there are some very serious questions about how we ought to behave towards the federal government. We need to think hard about appropriate guidelines to be used in seeking federal funds. Engineers have always enjoyed a good reputation for telling the truth. We are low on visibility but high on credibility and I think that is one of the reasons why this community is one which is fun to be part of.

This talk is supposed to be about the role of computation and experiment and in this connection I would like to mention one collaboration which illustrates the possibilities for bringing these two approaches closer together and also illustrates one of the dilemmas faced by experimentalists. I am referring to an effort being led by John Watmuff on the experimental side and Philippe Spalart on the computational side. Watmuff's measurements are in a low Reynolds number adverse pressure gradient boundary layer. It is an experiment which is specifically designed to match the computations being carried out simultaneously by Spalart in the CTR. The Reynolds number based on momentum thickness is on the order of 1500, which is comparable to the computation.

A disturbance is applied near the boundary layer origin and then a highly automated x-wire system is used to traverse the flow and follow the disturbance velocity field downstream. One of the interesting results that came out of this study is the fact that the disturbance can be followed for a very long way downstream even though the amplitude becomes extremely small compared to the surrounding fluctuations. The system is automated in that the computer

decides where the probe should be located and carries out a traverse which may last two or three days during which the experiment is running essentially hands off. The result is a very extensive set of data comprising on the order of 600,000 grid points. The reason I point out this work is that it is one example that I am aware of where a genuine attempt has been made to match the conditions of experiment and calculations.

In addition the highly automated aspect of Watmuff's experiment is food for thought when we begin to look for ways to share experimental resources in the future. It is clear that funds for large flow facilities are going to be very limited in the future. For turbulence research to have a healthy future we will need a facility for carrying out basic experiments at high Reynolds number. It is unlikely that more than one such facility would be constructed. Therefore it is important that workers have access to the facility through remote links, not just to receive data, but also to manipulate the apparatus and carry out individual experiments. This will require a high degree of facility automation and close coordination between the remote user and the on-site facility staff. There are precedents for this type of coordination in the field of space exploration where a single space probe or satellite is used to perform a variety of experiments for a community of users. At least some piece of experimental fluid mechanics research will need to adopt this type of approach in the future.

I would now like to talk about a project which is essentially computational but has some experimental features to it and suggests an important line of inquiry for experimental research in the future. It concerns a study of fine scale motions in low Reynolds number simulations of turbulence. The problem of the fine scale motions in turbulence is a defining problem in the sense that it is important for our theoretical understanding of turbulence, it is the key to improved subgrid scale models of turbulence and yet it lies beyond our current experimental capability.

This is a study of the simulation of the incompressible time-developing mixing layer carried out by Rogers and Moser, both of the CTR, for a Reynolds number based on shear layer thickness of 3000. The calculation goes on for a long enough time that there is one pairing event and then there is a breakdown to three-dimensional fine scales. We got interested in this simulation over the summer in the context of some questions we had concerning the local topology of fine scale motions. The goal of our work was to go in and study what kind of flow topologies exist inside regions of high dissipation. This is a question which you cannot ask of an experiment today.

What we tried to do is see what kind of topology characterized these regions. The approach is actually quite straight-forward; there is no conditional sampling involved, there's no thresholding. It is just a matter, first of all, of computing the instantaneous velocity gradient tensor. From that, we can determine the symmetric part: the strain tensor, and the anti-symmetric part: the rotation tensor. We do this over the whole flow and then at each grid point, we simply evaluate the three invariants of the velocity gradient tensor. The first invariant, P , is the trace and is zero for incompressible flow. The second invariant, Q , is related to a balance between straining and vorticity. The third invariant, R , involves triple products of the velocity gradients. The idea now is to take this tensor and derive from it the vector field which is associated with it. If you are talking about the velocity gradient tensor, that vector field is effectively the local velocity field. If you are talking about the rate of strain tensor, the local vector field is the velocity field as it would appear with rotational parts removed. In any case only a small number of flow topologies can occur as shown in Figure 1. In this presentation we will focus on the rate of strain tensor which has real eigenvalues and for which only two local flow patterns can occur (stable-node-saddle-saddle and unstable-node-saddle-saddle).

The second invariant of the rate of strain tensor is directly related to dissipation, the more negative Q_2 , the higher the dissipation at that particular point. A scatter plot of Q_2 versus R_2 is shown in Figure 2 (Chen et al.) for the Moser-Rogers computation. As can be seen both topologies are quite possible. However those points which are characterized by high dissipation (large negative Q_2) show a strong preference for the topology unstable-node-saddle-saddle. Or, to put it another way, for these points, the third invariant appears to be a function of the second invariant. These observations are crying out for both theoretical and experimental support. We don't know yet if this is a universal trend, ie, if it is the same for wakes, jets, plumes and so forth as for the plane mixing layer.

I believe that the future health of fluid mechanics research into the early part of the next century depends on the development of high Reynolds number experimental and computational facilities and on the development of experimental techniques which enable the measurement of velocity gradients over the full range of scales. On the computational side it is likely that, through advancements in local workstations and networking, most researchers will have access to adequate computational resources and they will be able to exchange their data freely with distant collaborators.

A number of people are developing advanced measuring techniques and it's important to match these techniques with comparably advanced flows: a very costly proposition. However, unless there is a significant increase in new funding for facilities, then computational research is likely to be in much better shape than experimental research. Excessive cost precludes the provision of distributed high Reynolds number experimental capability. We just don't have the dollars to support construction of a large number of new flow facilities.

So, I want to make the following proposal. We need a national high Reynolds number facility for fundamental fluid mechanics research. Emphasis in the design of the facility would be given to providing researchers remote access to the facility not only for the receipt of data but also for the manipulation of experiments in coordination with an on-site staff. The operation would be somewhat reminiscent of the way planetary experiments are carried out today. Existing NASA facilities which have been made available are a useful bridge, but I think that if our field is to be healthy in 20 years, we need a new large facility devoted to fundamental work. Given the magnitude of the Reynolds number required to advance the field, it is necessary to think in terms of cost which approaches 1 billion. Surely the goal of improving our fundamental knowledge of high Reynolds number turbulence is as lofty as that of improving our understanding of the behavior of matter at high energies. And, in the long run, the practical benefits to mankind are likely to be greater.

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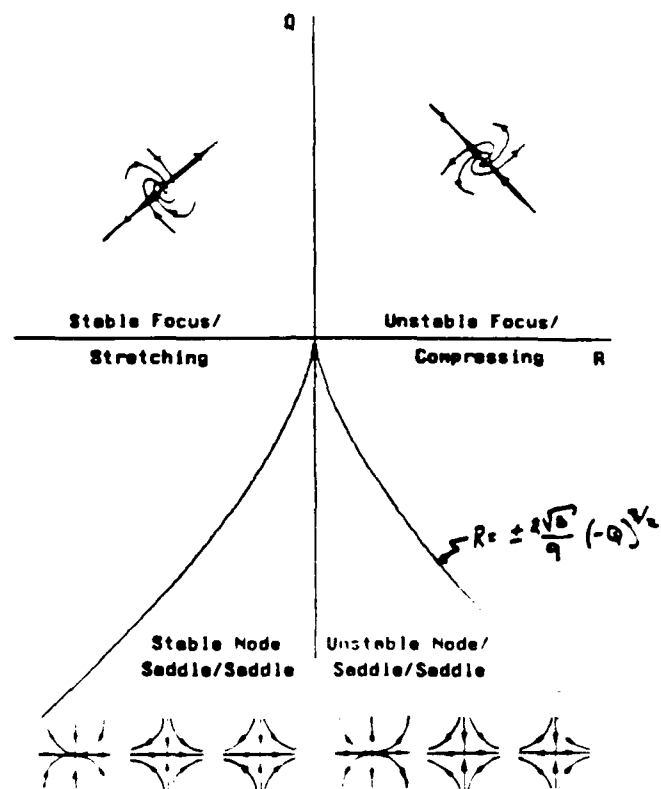


Figure 1

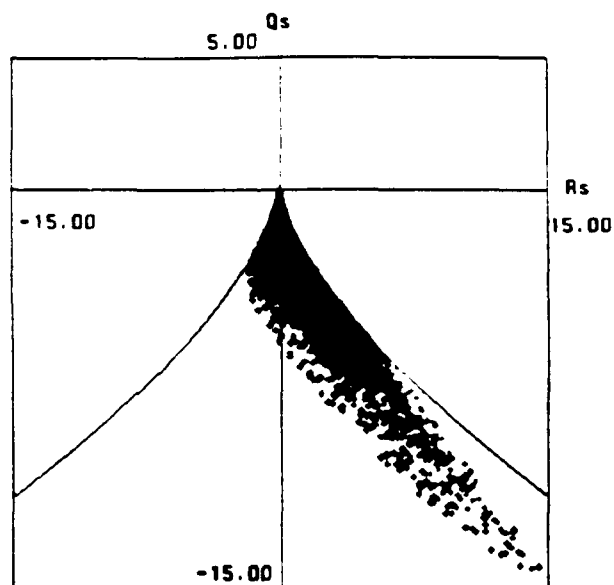


Figure 2

Steve Robinson (NASA Langley Research Center):

We all have experienced the question: "What is it that you really do?" But a friend of mine, an arts major, asked the really hard question: "How do you know when you are done?" There were 4 questions in the handout we got before the workshop began. Number 1: Are computers making experiments obsolete? Number 2: Can we anticipate a future where interactions between experiment and computations will be possible? Number 3: How can we deal with the increasing size of our datasets, the increasing complexity of our data analysis, and the difficult task of displaying time dependent and three-dimensional data? And number 4: How will we deal with the exchange of data between research groups? Most of what I have to say will deal with those questions. My remarks will be very brief - I have no data to present. There will be so much said during this workshop, I think that if I say less, more will be remembered.

Are computers making experiments obsolete? In some cases I would say yes, but not the experimentalists. I am biased, of course, but to me experimentalists have a unique way of looking at physical processes. Computationalists often, although not exclusively, come from a numerical analysis or computer science point background, while experimentalists are sometimes more tuned into the physical processes which they are trying to be understood; perhaps because they work with "exact" solutions of the Navier-Stokes equations.

For several low Reynolds number flows, it seems to be a consensus that we should not start new experimental programs for the same cases we can simulate. That doesn't mean we are done studying those flows, it just means we will probably study numerical data instead of experimental data. Historically, much of our turbulence knowledge comes from pipe-flows, channel flows, and boundary layers. The simulations are rapidly outrunning this experimental knowledge-base, and this will turn the focus back toward experiments as we progress to understanding more complex flows that are beyond the reach of simulations.

Nowadays, the experimentalist's horizons are expanding, and experimental analysis techniques can be applied to turbulence data from the supercomputer as well as the laboratory. From the success of the CTR summer programs, we can see the value of "experimental" analysis of numerical data, and this activity should be promoted wherever numerically-simulated turbulence data is available.

But let me caution you. Even for the flows which can be attacked with DNS, simulations cannot provide all the data you might want to study. Experiments, even in the simplest cases, are at the moment still the best for temporal analyses, which are necessary for studies of turbulence dynamics.

The second question concerns the interaction between experiments and computations. The interaction between experimentalists and computationalists clearly needs to improve; these two types of people must look towards each other and learn more about each other's tools. But I don't think the process will be entirely two-sided or fair. I believe that the experimentalists must take the responsibility for crossing over into the CFD field. It is more reasonable to

expect experimentalists to learn to use codes and analyze computational results than it is to expect CFDers to learn to use wind tunnels.

But computationalists also need to think about computing experiments. Generally the experimentalist and computationalist both try to approximate an ideal flow case. But boundary conditions, upstream histories, and free-stream characteristics can be considerably different between the two approaches, and we can wind up trying to compare apples with oranges. In some cases, though, current computational tools are capable of including some of the laboratory realities such as wind tunnel walls and model supports.

The third question for this workshop asked "what to do about these large datasets?" Numerical simulation databases are huge and highly complex in terms of the number of interacting types of motions. How do we extract only the enlightening information; how do we know when that is enough, and how do we know when to stop? By "capturing" a chunk of turbulence through numerical simulation, we presumably believe that all the answers appropriate to the simulated flow are contained in the captive space/time box of data. So why don't we understand turbulence, at least for simple cases?

I think part of the answer is that we are uncertain as to the questions we really want to ask of our captive turbulence. Part of this problem may be that many of the statistical tools with which we analyze and characterize turbulence were developed under the constraints of experimental techniques that couldn't supply nearly the volume and density of information as can numerical simulations. As a result, we often end up looking at a very rich set of data with a narrow-minded set of tools. The new wealth of computer-provided turbulence information should drive us to further develop new analysis tools, which will in turn allow us to ask classes of questions we may not have considered before.

What about the issue of exchanging databases? In my opinion, this is not nearly as big an issue as the fluid dynamics problems we face.

I think developing a standard format for data storage could require a lot of time to be spent for little return. To me, the main issues here are computer networking and data compaction. Clearly it is difficult to swap databases without a common network between the swapper and the swappee, and the slow arrival of networked computer systems in some experimental labs especially have affected some researchers' access to numerical databases.

Digital data compaction is common in the imaging sciences; even the representation of a computational domain in spectral space could be considered data compaction. But to store and work with the increasingly massive simulation datasets, digital compaction schemes should become commonplace.

To summarize my thoughts on the topic of experiments versus numerical simulations, I will begin by saying that the current ability of computers to provide numerical turbulence, even at low Reynolds number, is a tremendous opportunity for all fluid mechanics researchers. Simulations offer a great and largely untapped potential for teaching experimentalists to perform "smarter" investigations, and to develop new measurement and analysis tools.

Again, however, the experimental point of view will never become obsolete. Even if CFD can solve every problem we can think of, the physical insight to be gained from going into the lab is something we'll never run out of use for. Experimentalists still need to be trained in the lab, even for flows which can be simulated in computers. Of course, the obvious fact remains that very few practical flows CAN be simulated now, and so we have some

confidence that all these wind tunnels are worth paying the rent on.

One of the most important things I have to say is that the existing simulation datasets need to be much more widely available. Nearly everyone who has worked with them has found the simulations to provide revolutionary new insights into fluid dynamics. The datasets are so rich in new information that it will require many more researchers to explore and exploit them fully. These results are a national asset and should be available to the entire U.S. scientific community. Therefore, I believe NASA must take the lead in making these datasets more available under some sort of generalized and "equal opportunity" program. When this happens, I believe most experimentalists will add computer simulations to their toolkits, giving rise to a new breed of experimental studies and blurring the distinction between "CFDer" and "experimentalist."

Chuck Smith (Lehigh University):

I appreciate the opportunity to make a few comments and give you some perspective on how I feel we should be approaching the evaluation of the large data sets that we do, and will, encounter in our examination of turbulence. Earlier today we heard from John Lumley, who made reference to the importance of developing simple physical models of complicated processes. He pointed out that if you have a clear physical picture in your mind of how something works, then you have some basis for predicting what can happen. This allows you to develop a "feel", as an engineer or a scientist, for how something will change. I think that part of our problem in the turbulence community is we have not yet developed such a physical picture.

We've heard a lot of advocacy for large experiments and large flow field measurements, and I think those are part of a viable program for studies of turbulence. However, one of the things we haven't spent much time on, and Steve Robinson made allusion to this, is how we effectively evaluate these large data sets once they are established. Many of the most common evaluation techniques employed are rooted in our past approaches. However, we need to develop new evaluation procedures that begin to consider how we can most appropriately interrogate large turbulence data sets.

What I will discuss today are some ideas which address not only the evaluation of large data bases in computers, but also the other topic of this session, which is the interaction amongst theory, computation, and experiment as they relate to the evaluation process.

One of the things I've been involved in over the past several years is the performance of simple "model" experiments through which we are attempting to understand some of the potential dynamics of turbulent boundary layers. You might call these physical simulations of key phenomena which are potentially relevant to turbulence production. I know that there are others who have been involved in similar types of studies. Based on my experience with both these physical simulations and the examination of fully turbulent flows, my feeling is that improved instrumentation/computers are only part of the answer to better turbulence experiments. Our main problem is a lack of creativity in developing appropriate experiments and methods of evaluation which will allow us to determine turbulent flow dynamics, as well as kinematics. That's a key word -- dynamics. Tony Perry employed a slide of a brain to emphasize that we have to think and plan much more intelligently and creatively as to how we extract physical information from our experiments. I personally think that we have to approach this evaluation process in terms of the key dynamic mechanisms of the flow.

To support this point, I wanted to cite some comments from a meeting that was held at NASA/Langley last week. Dennis Bushnell said, "There seems to be a great concern over the development of improved methods of displaying turbulence results --- but how does this help us interpret the flow? What we really need are ways to understand the physics in a step-by-step process." He emphasizes that graphical illustration of data is nice, but only if it can help in understanding the step-by-step physics of what is taking place. I think you've seen an example of how one might move in such a direction in Brian Cantwell's presentation.

Fazle Hussain made a point that, "the result of most experiments and computations of turbulence is a vast amount of kinematic data --- to make progress in understanding this data, we must evaluate potential models of the physics and test them." Steve Robinson made a similar point, in that "to study dynamics, you have to be able to develop a model and examine it comparatively against available data." I think in many cases we've developed a model but we haven't necessarily tested it. And Seymour Bogdonoff said that "if you don't understand a flow, then you can't control the flow." I think we're still in the stage where we don't understand the flow, even though we have massive amounts of turbulence data in various forms, including the NASA/Ames 3-D, temporal data sets of Spalart.

For example, I've read a number of evaluation papers and had a number of discussions with people who have done detailed examinations of the data sets from NASA/Ames, and it seems that there are a broad variety of different interpretations that one can make depending on how you examine the data. This suggests that we still haven't focused on the question of how one most effectively seeks proper interpretation and hence understanding of turbulence.

That brings me to a brief review of how the process of studying turbulence has changed over the years. In the past, say 20 years ago, we made the intuitive assumption that turbulence was a stochastic process which led to our use of Reynolds averaging in our model formulation. We filled in the gaps in the modeling process by developing closure models from pointwise experimental data. In general, these models did a fairly decent job, although the closure techniques and empirical algorithms were very particularized in their effectiveness. With regard to turbulence control, the approaches were basically cut-and-try, with experimental measurements confirming the effectiveness/non-effectiveness of our "guesses" at control mechanisms. Development of control approaches were laborious and expensive, because we didn't understand the dynamics of the process, and thus could not rationally design effective control techniques.

If I consider our present process for studying turbulent flows, we have pursued approaches centered around the perception that turbulent flows are comprised of one or more flow "structures". Garry Brown reminded us that vortices form the muscle or sinew of turbulence. Clearly, he has in mind that the nature and distribution of these vortices are very important as a flow structure. However, we have not progressed very far in the characterization of the relevant dynamics of the respective flow structure, be it vortices or other flow elements, and this inhibits development of appropriate physical models and governing equations employing flow structure concepts for use in turbulence prediction. I think that this development of an appreciation for the dynamics of the flow structure is a critical missing link. And clearly, if we do not understand the flow structure dynamics, we cannot effectively design control processes. I actually see an opportunity here for using preliminary control approaches to help teach us more about the dynamics of turbulent flow structure via a feed-back process.

This leads me to an analogy of our methods of study of turbulence, which I call the "we can't see the trees for the forest". Part of this analogy has its origin in my frustration of trying to explain to my grandmother the complexities of turbulence. Consider an extra-terrestrial coming to earth and who, while hanging around at the edge of the atmosphere, observed a forest for the first time. Without ever landing to find out that the forest is composed of trees, he might try to determine what such an entity was using various electromagnetic wave measurements. Using his instrumentation, he might do all sorts of interesting things to categorize how the forest behaved. Given enough time, he could measure and characterize the undulating irregular behavior, the spreading rate (and angles), its ability to regenerate, and the susceptibility to environmental effects, to mention a few

global characteristics. These data might give him some limited capability of predicting some of the crude behavior of the forest. But if he really wanted to understand how and why a forest behaves like it does, he would have to land and determine that the key structure of a forest is a tree. Once recognizing that fact, he could then establish the properties of individual trees, such as their modes of interaction and collective behavior, their controlling properties, and their sensitivity and response to these properties -- in other words, the "dynamics" of a tree. Clearly, armed with an understanding of the dynamic processes of tree behavior, the development of a dynamic model of a forest for effective behavioral prediction and control would then become relatively straightforward. I can summarize this analogy as follows: We have to develop effective ways to hypothesize, examine, and relate the behavior of trees (i.e. flow structures) to the forest (i.e. turbulence) -- then we can approach the development of a truly dynamic model.

So how do we establish the important flow structure dynamics? I don't think it will be done by conventional direct evaluation of full experimental or computational data. We have always thought that if we had these large 4-D data sets, as we are starting to acquire, then we'd really be able to solve the turbulence problem. And lo and behold, now we are starting to develop these very large data sets, and we're having extreme difficulty understanding what is going on. We can make plenty of observations and interpretations of the kinematics (i.e. this is related to that, this moves in this way, etc.), but we have not been able to establish the dynamic processes that yield and control the observed kinematics. I believe that such an understanding and synthesis of the relevant dynamics requires a combination of experimental and theoretical/computational evaluation of what I would call simple "physical simulations", where one examines a hypothesized flow element which is much simpler than the overall turbulence, but can be the source of the relevant dynamics in a turbulent flow. Such flow elements may still be complicated (i.e. 3-D, time-dependent), but reduced to the essence of the central element of a more complicated process. I believe that Fazole Hussain referred to such flow elements as "kernels". The key concept is then to both study the dynamics of potential flow structure "kernels" and develop methods for establishing their relevance to the overall turbulence process -- essentially a study of potential turbulence "trees".

I've given some thought as to how one might most effectively utilize physical simulations in order to best develop an understanding of turbulence dynamics. I think that this process has to be very interactive, employing a close cooperation between experiment and theory/computations. Experiments allow you to explore a broad range of behavior and parameters quickly, but often are limited in their ability to determine certain quantitative information that can be obtained more easily from theory and computation. Theory/computation, on the other hand, allow detailed examination of spatial/temporal details, but are often limited in the accuracy of their simulation, and often cannot properly simulate more complex, 3-D behavior. The key point is that proper simulation efforts need to be a symbiosis of experiment and theory/computation.

From my perspective, there are five phases to the proper employment of physical simulations. The most important of these deals with the development of the basic hypothesis of the "structure" model itself (i.e. the conceptualization of a turbulence "tree"). Ways that have been used in the past include interpretation of visualization data and feature extraction. As an example, Head used flow visualization to evolve a hypothesis of hairpin vortices as a major constituent of turbulent boundary layers. Others have hypothesized other structures of perceived importance based on both experimental and computational flow visualization. Steve Robinson's examination of computational data bases has revealed different types of kinematic structures which could also be of dynamic importance.

Feature extraction is another technique from which hypothesized structures can be drawn. Examples of feature extraction are as subjective as Theodorsen's use his own intuition to suggest the hairpin vortex as model of turbulence, to the use of more quantitative methods such as Ron Adrian's use of stochastic estimation, and John Lumley's use of orthogonal decomposition. These latter approaches are examples of methods which selectively extract repetitive characteristics, which may or may not represent the dominant flow structure, but at least provide a hypothetical starting point for use as physical simulations.

The second phase of the simulation process is the development of methods which can translate the conceptual hypothesis of a flow structure to a viable "model" which can be examined either experimentally, computationally, or both. This often may not be particularly easy to do, despite the concept of a simple model. For example, Bob Falco has utilized a ring vortex as an example of his concept of a "typical eddy." My students and I have developed several ways to experimentally examine hairpin vortices, and Dave Walker and his students have labored long and hard over the proper simulation of the same type of hairpin vortices, only theoretically/computationally. Note that the effective development of physical simulations is often quite difficult, despite the apparent simplicity of a model.

Once you have established what and how you want to simulate, the next step is the examination of both the kinematic and dynamic behavior. This is where experimental-computational-theoretical interaction becomes a key factor. As I pointed out before, experiments and theory/computation are complementary in the insight they bring to an examination of the kinematics/dynamics of physical simulations. The following slide illustrates that complementation.

One of the things which Dave Walker has been very concerned with for a number of years, is what happens when you bring vortices into proximity of a surface. This physical simulation, by the way, has its origin in the pioneering studies of Bob Brodkey and Steve Kline, which indicated that vortices might play a key role in the stimulation of eruptive behavior near the bounding surface of a turbulent boundary layer. Dave has spent a career looking at such vortex interactions. As shown in this comparative slide (Figure 1), the theoretical/computational simulation of a vortex near a surface reveals a very rapid growth and deformation of the displacement thickness near that surface (upper part of Figure 1). As time evolves, the surface fluid supposedly will focus so sharply, that it in essence "erupts" from the surface in narrow spires. Is this important? Does it really exist? Well, this created an opportunity for an experimental collaboration to determine if such a type of behavior can occur. What we found experimentally, by use of a controlled trailing vortex and detailed hydrogen bubble visualization, is essentially the same kind of behavior Dave predicted. As illustrated in the picture (lower part of Figure 1), a streamwise vortex (shown in cross-section by a light sheet) causes a sharp, spike-like eruption from the surface which is markedly similar to the computational simulation shown on the left. What is the point of this comparison? This help us understand both the kinematics and dynamics of how a vortex in proximity to a solid surface can stimulate very narrow eruptions, a process that can have significant importance in understanding how momentum can be transported within the wall region of a turbulent boundary layer. Reflecting back on Tony Perry's comments, this simulation now provides us with a bit of dynamic understanding, which allows us to develop a mental image of the dynamics of a potential turbulent flow structure which will help facilitate a more effective assessment of turbulence dynamics.

Once you can effectively characterize the dynamics/kinematics of a flow structure simulation, the third phase of the simulation process is the utilization the simulation results for 2-D and 3-D correlation studies of massive turbulence data sets in order to establish the

viability of the simulated flow structure as an important turbulent flow structure. I call this process "matching." Using characterized property patterns of the flow structure "kernel," the corresponding turbulence data set is mathematically scanned and examined for goodness of fit of the "kernel" pattern, or template. Lu and myself have recently employed such a template matching process for hairpin vortices, and we have been able to establish statistics on relevance and magnitudes which are consistent with Tony Perry's hairpin hierarchy model. I believe that Ron Adrian is also employing similar methods using templates based on his PIV data. However, I think we have only scratched the surface in applying simulation matching techniques for interpretation on turbulence dynamics.

A fourth phase of utilization of physical simulations is in the development of physical models of turbulence via synthesis of the simulation results. I call this approach "patching", since you superpose together (i.e. synthesize) one or more simulations in an attempt to simulate the structure of turbulence on a more simplistic level via the cumulative effects. This approach is particularly attractive, since it can allow the staged examination of cumulative dynamics and potential examination of various environmental effects such as pressure gradient, roughness, etc.. I can think of several examples of work which employ this concept of patching. Dave Walker has incorporated some simple temporal models of wall region structure into very effective turbulent boundary prediction programs. Tony Perry has developed a hairpin hierarchy modeling approach which closely emulates turbulent boundary layer statistics. And John Lumley has utilized orthogonal decomposition features to synthesize a turbulent boundary layer prediction procedure. In all these cases, the cumulative synthesis of some form of extracted flow structure or feature was "patched" to yield a more complicated simulation of the hypothesized turbulent boundary layer.

A fifth phase in the physical simulation process is the utilization of simulations for examination of turbulence control concepts. Once developed and proven to be a viable constituent of turbulence, it is a relatively simple process to assess the sensitivity of physical simulations to various environmental influences. For example, it should be relatively straightforward to examine the modifying effect that a passive surface, such as a riblet surface, will have on a physical simulation, thus inferring the effectiveness of that surface on a fully-turbulent flow. Utilization of simulations also allows you to test conceptual ideas for their effect in modifying basic dynamics or physics of a flow, such as the examination of the modification of a controlled vortex-surface interaction by introduction of polymer additives. Obviously, there are many others examples of how simulations can be employed to examine potential methods of turbulence control.

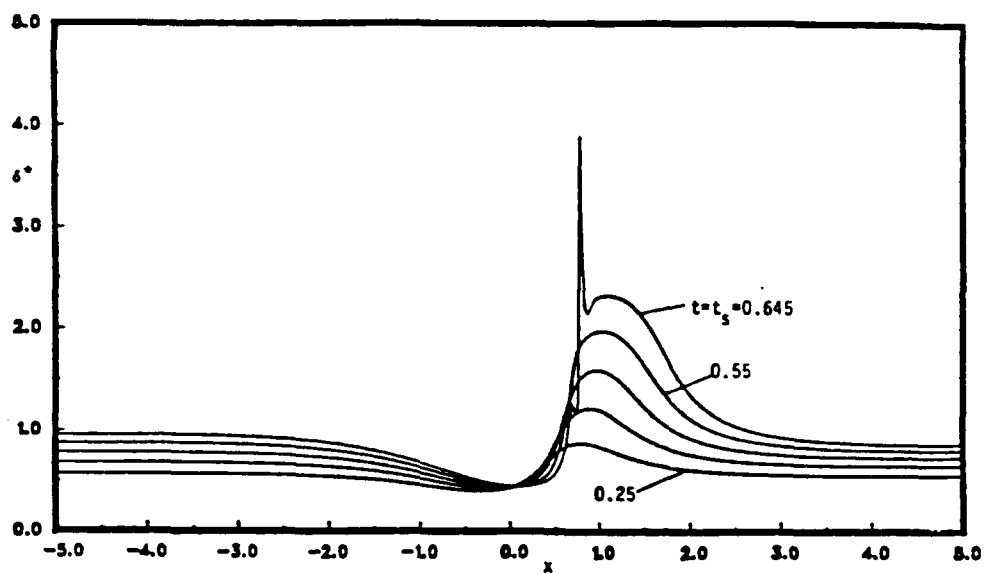
I'm going to conclude by saying that regardless of how well we are able to measure the properties of a turbulent flow, that we will make little progress in understanding what our measurements mean unless we develop and employ techniques for establishing the controlling dynamics hidden within the morasses of data we measure. It is clear to me that the utilization of physical simulations, and the development of associated matching and patching processes, are the direction we must move in order to allow proper assessment of large data turbulence data bases. And it is even clearer that the determination of the appropriate dynamics of turbulence through the use of physical simulations requires a symbiosis of experiment/theory/computation.

As experimentalists I think we have certain clear directions which we must take. I would suggest that we must:

- * Develop close working relationships with complementary theoretical/computational colleagues. I think it's fine if we can gain experience in theoretical/computational areas, but

I think it's impossible to be adept at everything. We need to develop relationships where we work very closely with theoreticians and computationalists. We heard Steve Robinson suggest that experimentalists (EP's, as he calls us) would be most effective at evaluating computational data. That type of statement bothers me since it suggests a one-way interaction, which is not interaction. We have to have an equal interaction; give and take on all fronts.

- Capitalize on the relative strengths to collectively examine the dynamics and kinematics of both physical simulations and turbulence data bases. There are different ways of examining flow dynamics. Certain studies are probably best done from computational and theoretical approaches; and there are certain things you can only do experimentally. We need to apply our strengths where they will be the most productive.
- Work collectively to hypothesize physical simulations of clear relevance and to develop methods for collaborative examination. We must determine what can be done, what is physically relevant, and how it can most easily be done.
- Develop creative methods for experimental synthesis and evaluation of physical simulations. It can be quite difficult experimentally to create many of the types of simulations we would like to examine. It's one thing for the theoretician or computationalist to say "let there be vortices", but this can be a much more difficult undertaking for an experimentalist.
- Establish and/or adapt techniques for determining the relevance of physical simulations in fully-turbulent cases. Some pattern matching approaches have been tried, but these are still relatively simplistic two-dimensional approaches. Other more sophisticated techniques utilized in electronics and robotics need to be explored and adapted. We have to become adept at image/pattern analysis in order to establish accurate comparisons.
- Exploit our understanding of physical simulations to design and optimize control concepts for fully turbulent flows. By using physical simulations to examine potential passive and active modification approaches, we can potentially validate and optimize an approach before we examine it in a turbulent boundary layer. This type of process has a much stronger physical appeal than brute force cut-and-try approaches.



**Theoretical/Computational
Displacement Thickness**



**Experimental Visualization
of Material Line**

**Figure 1. Experimental--Theoretical/Computational Comparison of
Vortex/Surface Interaction**

Discussion:

Ian Castro: I would like to ask Brian Cantwell if it is the health of the fluid mechanics community that depends on the large-scale facilities or just that of the turbulence community. Turbulence is only a subset of fluid mechanics and this would, therefore, not be true in general.

Brian Cantwell: I agree it is just the health of the turbulence community I was referring to. We do need to get to Reynolds numbers beyond what we have today if we are going to pursue turbulence research --- through more challenging boundaries instead of spreading out where we are in the Reynolds number range.

Anatol Roshko: That implies that we can handle flows at lower Reynolds number --- you don't mean that do you?

Cantwell: We do need to improve on our present capabilities. For example, the Rogers/Moser temporal mixing layer simulation really needs to be done spatially. But, 20 years down the road, the computational capabilities will perhaps let us handle these, and perhaps more complex flows. In 20 years time we should be able to compute low Reynolds number flows, even complex flows, adequately on a routine basis.

Roshko: Will these be direct simulations? Laboratory experiments have been doing direct simulations for a long time but the turbulence problem does not go away. I am not clear as to why you think it will go away now.

Cantwell: Spalart's boundary layer simulation, with all its limitations, goes up to Re_θ of about 1,400. If in 10 years we are not simulating boundary layers at Re_θ of 14,000, then something is wrong. Re_θ of 14,000 is at the upper end of a lot of the current experiments, although there are experiments run at Re_θ of 50,000. In 20 years time, we should be able to calculate boundary layers at Re_θ of 50,000.

Steve Kline: As far as the structure of turbulence problem is concerned, the fact that most of the data is at low Reynolds number should not matter too much since the structure will remain the same at higher Reynolds number. For example, the boundary layer streaks are present and riblets work at very high Reynolds number --- this should have been expected anyway. As far as national facilities are concerned, apart from high Reynolds number, do we need national facilities for studying complex effects such as curvature, roughness etc...?

Cantwell: Probably not. The high Reynolds number facility would only be used for carefully chosen problems and any practical problem would involve roughness anyway. The facility would probably not be used for extending the C_f range on a flat plate. Problems exist in fluid mechanics where we don't even know what constitutes high Reynolds number. If you consider the C_d versus Reynolds number variation for a circular cylinder or sphere, the C_d is still changing at a Reynolds number of a hundred million --- so is this the beginning of the high Reynolds number range or the end! So one experimental problem that could be

investigated in this facility is the behavior of the transition point on an advanced technology smooth cylinder. If, with increasing Reynolds number, the transition point keeps moving forward till it gets to the nose then no Reynolds number is high enough. But, if it stops at some point, then this constitutes a high Reynolds number for this problem. This would be a typical use of such a facility --- to figure out what constitutes a high Reynolds number for a given problem.

Kline: Do you see this high Reynolds number facility doing more than turbulence research? For example, drag and such applications would give it more justification. Also, are you implying that the kinds of effects that have already been looked at, such as roughness, three-dimensionality and pressure gradients would be done better in this facility?

Cantwell: Absolutely. I do not want to screw myself. As most of us here, I consider myself to be a small-scale researcher. We can still do significant work in small-scale facilities, unlike the high-energy physics people. But, in 20 years from now, the list and diversity of such experiments will be diminished and if we are not pushing the high Reynolds number frontier, we are going to be in trouble.

Roshko: I am both a devil's advocate and supporter of the high Reynolds number facility. I am not sure that increasing the flat plate Reynolds number by an order of magnitude will not be useful. One can imagine that the intermittency becomes so large at high Reynolds number that things become clearer.

Cantwell: We have no understanding of infinite Reynolds number solutions of the Navier-Stokes equations, except for a few astatic cases which are unstable. We do not know anything about the very high Reynolds number character of the solutions of the equations.

Roshko: And we must not lose sight of the fact that we cannot predict the flow over a circular cylinder at a Reynolds number of 100!

Lex Smits: I think Brian Cantwell's proposal is worthwhile, but high Reynolds number is not the only problem in turbulence.

Cantwell: John Lumley had a nice list of problems that need to be looked at. The list encompassed the whole breadth of fluid mechanics problems with turbulence. Every one of those problems is important, including chemical reactions, complex geometries, and so on. We would not want to close down fluid mechanics research in order to build such a facility, as the high-energy physics people did.

Karman Ghia: We only seem to talk about DNS (direct numerical simulations) when computations are mentioned. It is unfortunate that LES (large-eddy simulations) has been pushed aside with the advent of bigger and faster computers. LES has a better chance of achieving high Reynolds number in the near future. People tend to forget that LES is still alive and ticking.

Cantwell: I think DNS has played a very useful role --- it has enabled us to probe turbulent flows in considerable detail. The results I showed earlier from the summer session of the CTR (Center for Turbulence Research) which suggest that the variance of the rate of strain tensor to the third may be related to the second at high Reynolds number in the neighborhood of highly dissipating regions --- those results, in my view, are crying out for a theory. It is conceivable that such a result could form the basis of a theory for the fine-scale structure of turbulence. This is the kind of thing DNS is capable of.

Bill George: I agree that we need high Reynolds number facilities. Answers from these facilities may prove to generate lots of ideas and a lot of things we take for granted may prove to be wrong and we won't know until we push towards higher Reynolds number. For example, in Spalart's calculation, he looks for regions where the velocity gradient is proportional to distance from the wall, and there is none. If it doesn't exist at higher Reynolds number, then we need to rethink our understanding of a turbulent boundary layer. Another example: a lot of our understanding is based on the idea of a tendency towards local isotropy of the small-scales as the Reynolds number goes up. Some evidence from Russian experiments at R_λ of about 3000 suggests otherwise --- the dependence on large-scales is never lost. So this may require a fundamental rethinking, as far as our picture of turbulence is concerned.

Cantwell: I think this business of promoting such a facility is easier than promoting the SSC (Superconducting Super Collider). Also, I envision this facility to be very highly-instrumented, very highly automated and very much on line, so that down the road, we can carry out experiments from our offices.

Hans Fernholz: Have you ever done such experiments?

Cantwell: The experiments by Watmuff I described earlier are a crude beginning to such experimentation. Jon (Watmuff) sets up the area to be studied and leaves it running for several days --- then starts to look at the results.

Dennis Bushnell: On the issue of constructing a large-scale facility, it is only done when there is a crying national need. In aeronautics, the last big facility was the NTF (National Transonic Facility). It cost between 80-120 million dollars and was constructed for testing the C-141 airfoil. There are not that many high Reynolds number problems in aeronautics --- in naval architecture, submarines get up to 30 billion, whales up to 2-300 million. The fuselage on a large aircraft is of the order of 3-400 million, but the NTF will go up to 6-700 million quite comfortably. Liquid helium tunnels are interesting --- if you really want high Reynolds number, a small liquid helium tunnel that fits in your basement laboratory will cost 5-10 million, in contrast to the 1 billion dollars for the large facility.

Cantwell: When you get into the details of a small liquid helium tunnel, there are other problems, such as the resolution of the fine-scales and compressibility.

Bushnell: If we need a facility to address the weather forecasting, for which there is a need, then we would need to include niceties to do with stabilization, Coriolis etc.. This would make it a very different facility.

Cantwell: In order to argue the case, and sell it to the public, we have to point to specific problems, such as global warming.

George: I have a comment. The problem with our discussions is that we haven't allowed ourselves to think about what a solution to the turbulence problem might imply. We think as engineers, wanting to fix the flow. We need to let our minds run --- what would constitute a solution to the turbulence problem? If we don't push the bounds of what we think we understand about turbulence now, we don't have a hope of understanding what that solution might be.

Kling: I would like Steve Robinson to elaborate on what sort of questions we ought to be

asking of the simulation data base. What should be different to what we have done in the past.

Robinson: From the experimental point of view, we need to learn to do better experiments. As an experimentalist swimming around the simulation data base, you come out with a different point of view of how to do experiments and which questions to ask of experiments. In terms of what else ought to be asked of numerical data bases, there is the question regarding dynamics. Both DNS and large-eddy simulations need to address the question of flow control.

Roshko: It seems self-evident that the more interaction you get between experimentalists and computationalists, the better things are going to be --- similar to what the CTR is providing.

Bert Hesselink: A statement was made that exchanging data is not a big deal. Actually it is a very big deal. Consider experimental and numerical simulation data to be distributed and estimate the size of the data base for fully-resolved, reasonably high Reynolds number turbulent flow. You get a very large data set which is not easy to transfer over networks. Secondly, we need to do something about data formats since translation is a big problem. We are usually going to extract only a small amount of data from a given set. Exchanging data sets and extracting data are prominent problems of the future.

Robinson: Data transfer is a time-consuming, effort-intensive project. I moved several gigabytes from NASA Ames to NASA Langley. It takes time, not deep thinking research time, but machine time. We can also borrow a lot of the technology (regarding data transfer) from other areas.

Jim Brasseur: I have tried to move Mike Rogers' mixing layer data from NASA Ames to Penn State in the last year. I was faced with a lot of difficulties, many of them seemingly trivial and stupid, but they existed. The size is big and you need a place to put the data. The format is another problem, not the structure --- that can be worked out, but the binary and physical format of the data. So it is certainly not a trivial process.

Chih-Ming Ho: We talk about large amounts of data. We need to develop 2-D/3-D global evaluation techniques for such large data sets. In the past, if I have one sampling point, I can get the time-averaged velocity, with two I can get correlations and with ten may be conditionally-sampled data. However, now we have ten orders of magnitude more sampling points and if we still use the old approaches, that is a waste of resources. The development of 2-D/3-D global evaluation techniques will help tremendously in understanding the newly available data.

Ghia: Even if the simulation data sets are readily available, as Steve Robinson suggested, researchers are not typically equipped to handle such large data sets on their PCs or super-workstations. Even the simulations we conduct ourselves on the Cray generate too much data to handle easily. Experimentalists are not quite ready to handle large data sets generated by places like NASA Ames.

Robinson: The problem will remain until the data sets are made readily available. What needs to be done is to make the data available in generalized formats, so that problems can be attacked on a wholesale basis, instead of specific problems being addressed on a case by case basis. It is not as simple as sending out optical disks to everybody, but if the data were available then people will develop means to study them.

Ghia: For example, the Rogers and Moser mixing layer simulation results that Brian Cantwell showed must contain a lot of data --- it takes millions of data points to get those pictures.

Cantwell: All that processing was done on a Personal Iris, although some initial processing was also done on the Cray. Once (read/write) optical disks become available, the transfer problem will be made a lot easier.

Kline: A lot depends on what you are trying to do --- one can take subsets of the data to look at specific features.

Don Rockwell: I would like to talk about global quantitative visualization. The overall objectives are to identify (in a general sense) patterns of unstable and turbulent flows, classify patterns and compare them with a library of reference patterns and respond with some control or modification technique.

We are now at an exciting stage of getting whole field measurements, using PIV and scanning LDV, for example. We also have established methods of describing flow patterns that have been very successful, such as critical point concepts, stochastic estimation and coherent structures. I would like to comment on the possible formulation of new types of what might be called economical shape representation and economical topological identifier. I don't really have any solutions, but what I would like to suggest is that we look at other technologies where they are worried about compression of data and recognition of patterns. There are constraints if we follow such a philosophy. We would like to allow compression by proper selection of some predominant feature of the flow pattern. The criterion has to be able to tolerate rapid changes of pattern shape in fluid mechanics and it should be rigorously invariant with orientation, translation and scale of the pattern. Such ideas are used widely in other technologies. For example, the identification of finger prints using chain-code concepts whereby a single feature is identified from a relatively complex pattern and then stored in libraries for future comparisons.

What are the other related technologies that may be used and how can they be exploited? There are clearly limitations when it comes to fluid mechanics since the problems are more complex --- deformation of the flow, lack of well-defined boundaries and so forth. But, there are advances in different areas that may be helpful. In manufacturing, they are interested in automated defect-detection at sub-micron levels using laser scanning. Real-time feature extraction modules have now been developed for PCs --- these modules use hardware to identify features quickly. In medicine, we are all familiar with Catscans --- 3-D imaging techniques and hard-copy holograms are also now becoming available. In vehicle guidance, various techniques have been developed for looking at temporally-developing imagery. Significant advances have also been made in optical recognition systems and robotics. In the general area of rather interesting developments are the so-called hybrid optical digital image recognition schemes --- these are optical schemes used on-line in real time. In the next 10 to 20 years, we need to take advantage of these technologies and come up with real-time feature extraction techniques and control mechanisms.

Gary Settles: That is an interesting point and we would all like to do that type of image processing. However, we have a long way to go between what is available now and what tools are needed to do sophisticated image processing just described.

Rockwell: I agree with that entirely.

Val Kibens: I would like to follow-up on the issue of the sizes of the data bases and how do

you know when the job is done. Any measurement technique will have a certain data flow rate at which information can be obtained about a process. By using sophisticated techniques on simple mechanisms, one can get "all" of the data, whereas using a simple measuring system on complex mechanisms gives an inadequate description.

When is the problem finished? How complex is the problem? What is complex and how do I know when I have described it adequately and how can I proceed? Let's look at this chart which plots perceived versus actual complexity. At a given time, the measurement technology gives a certain database size. We have to act on that database --- this should be done according to where the problem falls on the chart.

Since we have access to a limited amount of information, limited by measurement technology or ability to proceed, we have to make our peace with basic experiments --- with something more complicated that becomes a system or with a general field of technology. In trying to put together a map, let us look at this graph of accessible information content plotted as a function of time and complexity level. In 1920 here I am working on a low Mach number jet in an experimental situation and this is the database I have managed to acquire. With time, I know I will be able to get more and more data --- I may well get to a point in the 1980s whereby there is enough data to describe the flow field in detail (e.g. coherent structures). So I have more than enough data to study the coherent structures, but what do I do with the rest of the information. Let us go to something more complex, more like a system. Let us take a given level of information content and ask what can I achieve with the system? I will not, by necessity, achieve some level of resolution that is less complex than dealing with a simple system. If I am in industry and I am dealing with an airplane, I may well find myself having to accept, because of the complexity of the system, an overall level of resolution that is extremely low and proceed nonetheless. So then this becomes a matter of strategy.

So I have to place myself somewhere in the space where I am considering the levels of complexity and basic and applied approaches in deciding what my next step is. Let us look at this graph of empiricism versus complexity in approach in research. So what can I expect/project? This is just an ad-hoc attempt to see what the relevant parameters might be for me, but I would expect that just as in the 1920s, I have to give up any kind of a basic scholastic approach at a fairly simple level. And I have to say that if I go to a non-linear problem, I have to go into an applied/empirical approach for this problem. Today I could probably carry the basic approach quite a bit farther in complexity, where complexity may be any number of things. At some point, I have to give up and I have to say that I have to do this in an empirical/applied fashion, and for this I have my development engineering department in industry. As it is, if I truly consider something important enough, I can expend all the money and resources I have and bring that one item down from an applied to a rather basic approach. This takes maximum effort, but I can do it, the resources are there. These kinds of maps, in my mind, would define for me what is possible, how much money I need to make progress and how do I know when I am at a milestone. It is a navigational map, the need for which I feel in trying to chart some kind of reasonable future path.

Brasseur: I have some comments on what Steve Robinson and Chuck Smith said earlier. I would like to comment on three topics: (1) Analysis as opposed to data collection, (2) Combination of experiments with simulations and (3) the role of analytical work --- in combination with experiments and simulations.

We have to separate the data analysis process from data collection. Simulation data are very complex and of such magnitude that they provide a stumbling block for the present techniques. This is especially true when we want to look at the data dynamically or

kinematically. The issue of extraction of quantitative analysis, rather than subjective visual analysis, needs to be looked at. This would be useful for creating conceptual models and understanding kinematic and dynamic processes. I have one application here. This is a vorticity field for a homogeneous turbulent shear flow (Mike Rogers' temporal simulation) and it shows iso-contours in 3-D through a 64 cube box. Each one of these areas represents some region of activity in the vorticity field. The problem is that we get a visual picture of regions where, presumably, there is some level of intense activity, but we can't quantify it. All we get is an overall picture and if we try to reduce the contour levels then the picture becomes so cluttered that we can't make any sense out of it. So there are two problems. One is that we are trying to extract subjective information and the second is that we can't quantify this very well. So one approach which we are taking is to actually try and select out the intermittent regions upon which these iso-contours are sitting. In other words, this is the top of the mountain, as it were, and somehow we want to define what the whole mountain looks like. We have developed a technique to do that. I won't go into any details, but it does allow you to do this sort of thing. We find that with this technique, you can actually map out the whole structure --- this is only a subdomain --- we can get very nice structures. Furthermore, we can define the volumetric extent of these structures, so we know exactly where the structures are in the dataset. Having done that, we can then go back to the data and we can quantify these things. We can do all sorts of quantifications with this --- we can look at the structure within these in terms of, say, the components of the vorticity field, we can look at the contributions to the statistics, we can look at the intense events, and we can do the same thing with uv and relate that to the vorticity. We can hence quantify everything we see. So what I am arguing is that we need to be thinking in terms of trying to quantify any notions we obtain subjectively. We also have the ability, once we have identified the regions in the dataset, to track them in time. The thing is that you know precisely where they are so you can quantify what you are doing as well as digitally extracting information.

The next point I wanted to make is the emphasis on analysis. We should think of data (experimental or numerical) collection on the one hand, and on the other hand think of analysis of the data. This not only provides a good mechanism for carrying out the work, but it also provides a basis from which people can work together who are involved in different types of data collection. For example, if you have a facility which is emphasizing analysis, which I am trying to create myself, then, in principle, an experimentalist can get together with a simulator and say lets design a joint collaborative experiment/simulation and in doing so, lets define those regions in which the experiments and simulations overlap. This is different from the concept of saying that simulations can handle low Reynolds number simple flows and so the experimentalists should not study them any more. I would argue that the experimentalist and the simulator should sit down together and say that we can do the same thing in the overlap regions, but we can both extend the boundaries in our own ways. For example, in DNS, we can do the full 3-D field and get a lot of information, whereas in the experiments the parameter space can be extended to higher Reynolds number or to more complex geometries. So there is a need for researchers to sit down together and think of quantitative tools that overlap. And, these should be designed such that each party can take advantage of their strong points.

Another point I want to make is on the role of analytical work. There is so much emphasis on complex experiments and complex data collected from the computer. The theoretician is viewed as somebody that is supposed come up with a model that predicts what is going on in the experiment. I argue for another role for the theoretician --- to pick out the essential physics in the process. If the process is complicated then we may have to limit the parameters and state that in this range of parameters there is some essential physics that is

taking place. And if we just pick out the essential physics and put it together then may be we will learn something that will reflect back on what to do experimentally or numerically. I have an example there as well. If you look at the structure of the Navier-Stokes equations, you find that there is triadic structure in Fourier space that tells you something about the coupling between modes. I have done some work which suggests that the large-scale structure is coupled, in some sense, to the small-scale structure so that if you do something drastic to the structure of the large-scales, it should reflect itself in something drastic to the structure of the small-scales. This is contrary to some notions of local isotropy, but you can see it in the equations. So I designed a numerical simulation for this. This is the small-scale enstrophy and we mix it up with the large-scales. We look at the peak in the energy spectrum and we start stirring it up with large-scale vortices. So after we do this for a while, the large-scales become more energetic and then we look to see what happens to the small-scales. After stirring the large-scales for a while we get these rectilinear vortices. When you stir the large-scales long enough the small-scales are very strongly affected by the large-scale stirring which is indicative of the coupling I was talking about. So the point I want to make is that the development of the numerical experiment was based on theory and there is a loop there that can be taken advantage of.

The last point I want to make is to do with the statement that Steve Robinson made that the experimentalist should learn CFD. In my opinion, that sort of statement is counterproductive. Everybody can learn from everybody else and everyone has a certain expertise. The best thing to do is to create linkages, take advantage of each other's expertise and work in a global framework.

Castro: Steve Robinson claimed that experimentalists should do the walking. Jim Brasseur suggests that both should while Tony Perry claimed the supremacy and skill of the experimentalist. Who should do the walking? What is the consensus of the group?

Robinson: I did not mean to assign supremacy at all.

Tony Perry: It would be terrific if you could have more people working in all these fields --- all that information sinking into one brain --- too many brains tend to confuse the issue.

John Wyngaard: I used to do experimental work long before DNS came along and we were content to measure what we could --- velocity, may be a vorticity component and some components of the velocity derivative. Now that we have been spoilt by the amount of information generated by DNS, how do we go back to high Reynolds number experiments where we cannot measure even a tenth of that?

Cantwell: The key to the high Reynolds number facility is to push techniques so that we can measure more quantities such as velocity derivatives and the development has to be funded. There are upcoming techniques which will have to be developed.

Perry: Regarding the high Reynolds number facility and looking at velocity derivatives, the implication is that as the Reynolds number increases, measurements become more and more difficult since the scales get smaller and smaller. This suggests that we should go to high Reynolds number by going big --- a meteorological boundary layer has a Kolmogorov scale of the order of 1 cm, which is pretty big.

Wyngaard: You are right, we should go slow and big rather than fast and small. Experimental techniques are still, to a large extent, a black art. If we put 1% of the money that goes to DNS towards experimental techniques then we would be more productive.

Hesselink: As for the size of the facility, I would argue for the opposite point. There is no fundamental limitation that says we cannot go to smaller scales. It is just that optics generally have a resolution of a micron or so, but there is no reason why other forms of radiation cannot be used to do the probing. The power requirements for a high Reynolds number facility scale as L^3 . So there is much to be said for making the facility smaller and putting our efforts in areas where other technologies have been developed, such as in micro-electronics and micro-optics. This underrates the idea of developing more instrumentation. We should go smaller and put resources in areas where problem really exists --- in the measurements. The basic problem is that the Nyquist sampling of the largest scales require you to average over several structures and so when sampling large volumes, one ends up with enormous amounts of data. So there are positive arguments for going smaller rather than bigger.

Roshko: Talking of fundamental problems, one of John Lumley's points was that the main limitation, for support purposes, is money.

Wyngaard: Jim Brasseur is making predictions about local anisotropy which we cannot check. (From audience: Why not?) Can you measure the velocity spectrum tensor in wave number space? That is his prediction. We couldn't even measure wave number spectra the last time I tuned in.

Hesselink: There is a difference between what we can do now and what we may be able to do. If somebody brought up a very important problem which had to be solved then it can be done, just like in the medical technology. If there is a need in fluid mechanics to measure a particular quantity, the we need to put our minds and resources into it and we can come up with some new techniques.

Rabi Mehta: Where will the money come from?

Hesselink: Where will the money come from for the high Reynolds number facility?

Smits: We should not use the high Reynolds number facility as a paradigm of what we are aiming for. Turbulence is a much richer field than being defined by a single goal of high Reynolds number. How do you define turbulence problems? We should not be too specific so as to alienate the public. On the other hand, a more general issue, such as pollution, would generally be considered an important problem. I have tried to come up with some examples identifiable with turbulence problems. They are neither too specific nor too general. Particle dispersion, pollution, environmental issues --- these are still turbulence problems. Each of these issues has a high Reynolds number component to it. They may also have a Mach number or Prandtl number component to them. I haven't talked about heat transfer, chemical reaction and combustion --- Reynolds number plays an important role in all of these. Another way to cut this problem is to follow what Chuck Smith and Fazle Hussain do and talk about kernels of identifiable motion which are relevant to turbulence and study those. For example, if we could study single eddies in isolation, will that give us a great truth that we can use in these other problems? May be a mixing layer is a paradigm of turbulence, or may be it is a boundary layer --- there is an awful lot of work done on boundary layers. Does that give us a key to bigger problems? And can we make that connection clear?

Bushnell: Most real flows are fully 3-D in the mean and act very differently from 2-D mean flows.

Smits: All these problems are 3-D, but we may want to study them in 2-D configurations if that is relevant to the problem.

Brasseur: Everyone watches the weather report after the news --- we should include meteorology to the list.

Cantwell: The billion dollar facility could well take the form of a huge convection cell --- this may be easier to sell to the community.

Perry: Don't forget oceanography --- that should also be added to the list.

Ghia: We need to relate turbulence issues to the human being --- that would be the key to success.

Smits: Everybody is upset at the price of oil going up, with the situation in the Persian Gulf. Our work can make a difference to the price of oil.

Ghia: I was referring to natural disasters and if turbulence research can be used to avoid these and in saving human lives.

Doyle Knight: It seems that the various scientific disciplines that have succeeded in obtaining funding have done so by identifying the major research problems. Examples are the National Institute of Health and Cancer, Department of Energy (fusion), Department of Physics (superconduction) and many more. Missing in our discussions today, apart from John Lumley's presentation) is the answer to the question: What are the major areas of research in turbulence? The answer to this question is a pre-requisite to answering other questions on the agenda. For example, we can't talk about what facilities we need, or what experimental tools, or computer algorithms, until we have decided what areas are important. I was distressed to hear about billion dollar facilities without hearing one main description of problems to be solved in the facility and why they are important. So I would like to propose that each person here writes down the answer to the following question: "The major research problem(s) in turbulence, in my opinion, is (are)" [This was done and the results were later presented and discussed.]

SESSION 3**Instrumentation and Facilities**

Session Chairman: Jean-Paul Dussauge, IMST Marseille

Session Recorder: David Wood, University of Newcastle, Australia

Bert Hesselink (Stanford University):

This contribution (pp. 52 -56) was not available in its final edited version at the time of publication. I hope to be able to send it to you as soon as it becomes available (Ed.)

Fred Browand (USC):

The focus of my brief presentation - prepared with the help of Geoff Spedding - will be two-fold. First, I wish to discuss a technique of data acquisition which may provide, over the next few years, an inexpensive and reasonably standardized procedure for acquiring velocity data in a plane. Such a technique might replace the hot-wire as the primary source of experimental information about turbulent flows. The technique is Digital Particle Image Velocimetry (DPIV) as utilized, for example by Willert and Gharib [1]. Second, I describe the wavelet transform, a recently developed data analysis tool allowing one to perform a localized Fourier analysis. In particular, I will discuss a new two-dimensional transform having useful properties.

DPIV requires the introduction of neutrally buoyant particles which follow the flow. The technique is probably limited to liquid flows. Rather than the frame-to-frame tracking of individual particle images to obtain discrete vector displacements, DPIV identifies the position of the peak cross-correlation computed in sub-areas of two images separated by a small time interval. The root idea has been traced back as far as Kovasznay and Arman (1957)[2]. It has been used in Japan by Kinoshita [3], and by Kimura and Takamori [4], as well as by Adrian and Yao [5] in this country. It was recently utilized in a complex turbulent flow by Utami, Blackwelder and Ueno [6]. What is so appealing about the method is that it removes the onerous task of identifying individual particles--whether it be by hand or by computer algorithm. (It is not necessary that discrete particle images be distinguishable.) In addition, the method can be operated as a real-time (or nearly real-time) velocity detector. Spatial resolution is limited as will be seen shortly, but these limits are based entirely upon detector technology and computer storage capability. The method will improve in the future with the inevitable improvements in CCD cameras and computers.

The method works as follows. Consider a discrete-array (video) camera image to consist of 512×512 pixels. Take two images separated in time by a short interval, Δt . Divide each image into sub-rectangular areas - perhaps 16×16 or 32×32 pixels. Cross-correlate the two corresponding sub-images (in the two frames), and interpolate the position of the peak in the (cross-correlation) distribution. Willert and Gharib [1] claim this can be done with an accuracy on the order of 10^{-1} or 10^{-2} pixels, depending upon the number of particles within the area. Thus the dynamic range of the instrument - the largest velocity measurable expressed as a fraction of the smallest velocity - becomes of the order of 10^2 to 10^3 . The spatial resolution - that is, the ratio of the largest scale to the smallest scale resolvable - for the 16 pixel sub-area is of the order of $512/16 = 32$, giving roughly 10^3 independent velocity estimates over the image area. Using a sub-area of 8×8 pixels doubles the spatial resolution, but at the expense of decreased dynamic range. Willert and Gharib [1] have made performance estimates as a function of particle density. These studies could be expanded. One interesting question is: How should one assess this method in comparison with other methods for velocity field determination?

An answer to the previous question would serve to classify the many competing procedures available. As a means for such classification, we would suggest the use of synthetic images

based upon the insertion of particles of specified properties into known velocity fields. The Burger's vortex is a simple example of a useful three-dimensional velocity field. Other useful test cases might consist of examples of actual flows. These could be kept in a common data bank for access by any interested party. For example, a researcher might want to test a new algorithm by utilizing one of the synthetic or "standard" flow fields. Alternatively, persons could receive the best algorithms presently available, together with information on resolution and accuracy. In this way, the superior methods would survive in documented form, and be available for community use.

The acquisition of accurate field data stops short of the desired goal. It is also necessary to process and display these large data sets in useful formats - which brings me to the wavelet transform. The wavelet transform represents the convolution between a localized wave packet, or wavelet, and a given space (time) series. For data analysis, the wavelet is frequently an oscillation contained within a Gaussian envelope. As the envelope is allowed to shrink or expand with scale a , the oscillation within the envelope shrinks or expands accordingly to preserve similarity of shape. The wave packet may resonate with local features in the space series having similar oscillating structure to produce large transform values. Where no resonance occurs, the wavelet transform takes on small values. The spatial resolution of the transform is proportional to the scale a (the scale of the envelope).

$$\sigma \sim a$$

In wave number space, the resolution is inversely proportional to wavelet scale,

$$\sigma_k \sim 1/a$$

and the resolution product $\sigma \cdot \sigma_k$ is a constant (Mallat [7]).

A wavelet family which is complex, and has a spectrum which vanishes for negative wavenumbers (frequencies) is particularly useful, for it allows the separation of amplitude and phase information. The most familiar family member is the Morlet wavelet.

$$g(x'-x)/a \sim \exp(ik_0(x'-x)/a) * \exp(-(x'-x)^2/2a^2)$$

The wavelet transform formalism is extendable to two space dimensions. For the Morlet wavelet, the extension is accomplished by making x , x' vectors, and regarding k_0 as a vector.

$$g(x'-x)/a \sim \exp(ik_0(x'-x)/a) * \exp(-(x'-x)^* (x'-x)/2a^2)$$

In the wavenumber plane with components k_x , k_y , the Fourier transform of the Morlet wavelet is a Gaussian hump of scale $1/a$ centered a distance $|k_0|/a$ from the origin, and making an angle $\alpha = \tan^{-1}(k_{0y}/k_{0x})$ with the x -axis. As a varies, only a wedge-shaped region in wavenumber space is interrogated. To sample the complete wavenumber space using the Morlet wavelet, a series of wavelets having different spatial orientations (different vectors k_0) must be employed. Frequently, two-dimensional patterns contain energy at many wave orientations. It would be useful to have a single wavelet transform which would encompass larger portions of wavenumber space. This is accomplished with the wavelet termed Arc, developed at USC by Thierry Dallard and Geoff Spedding. Arc is defined in wavenumber space as a semi-circular ring planform having a Gaussian-shaped cross section of width $1/a$. The center of the ring is located a distance $|k_0|/a$ from the origin. The orientation of the semi-circle is free to be chosen, and might be taken as either zero or $\pi/2$. The ring expands and contracts as a varies, and a complete half space is covered. It is sufficient to

cover the half space, since the spectrum of negative wavenumbers is redundant for a real function $f(x)$.

In conclusion, a series of color slides are presented to illustrate various properties of the Arc wavelet transform. One example is a test function consisting of the sum of a desired 2-D pattern centered at wavenumber k_1 , a subharmonic pattern at $2k_1$, and random noise. The subharmonic and the noise each have rms amplitudes a factor of ten larger than the desired pattern, which is not visible in the summed field. Filtering with the wavelet Arc at the appropriate scale recovers the desired pattern. The other example illustrates the use of Arc for analysis of data from an acoustically forced mixing layer. The development of a localized change in scale (wavenumber) is followed as a function of downstream distance. Application of the wavelet transform allows the definition of the boundaries of the domain and supplies precise energy levels for the various scales within the domain.

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Discussion:

Tony Perry asked whether Bert Hesselink thought it is possible to measure all nine velocity derivatives simultaneously at all points in a flow.

Hesselink said that there are enough degrees of freedoms to allow optical systems to do this.

Steve Kline asked about the cost.....

Hesselink described a system to get a Lagrangian measure of the vorticity which would not be much more complicated than a conventional LDV system. Further details can be obtained from him. You need a photochromic fluid and good optical access to the flow to get all nine components. The nice feature of the system is that the optics take the derivatives directly.

John Wyngaard asked about the prospects of measuring "sub-grid" scale fluxes in turbulent flows, relating these to the grid-volume-averaged velocities and proceeding to turbulence model development.

Hesselink said his inclination is to say yes, something can be done. The system he just described has been used to make measurements at $y^+ = 1$ in a boundary layer, and has the right ingredients to do what was asked.

Fred Browand asked for details on Hesselink's micro-windtunnel proposal.

Hesselink said it is in the conceptual stage. Micro-devices can be made using silicon. From this could be built a tunnel of dimension 1 mm or smaller with small mass flow rates and micro sensors. It would need only small forces to control it as natural frequency goes as volume.

Tony Perry asked what would be the Reynolds number?

Hesselink said micro-Reynolds number. His proposal was not meant to replace the "billion dollar wind tunnel" proposal. The only reason he brought it up is there are technologies which can provide solutions to fluid mechanics problems but which are being ignored at the moment.

David Williams pointed out that the bottleneck with digital particle image velocimetry (DPIV) is in the time resolution ability and processing time required. It is not feasible to get spectral information in real time. Are there any breakthroughs?

Browand had no additional information. The point is that DPIV allows interaction with the experiment; its attraction is that the experiment can be changed on the basis of what you see, but this does not address the need for post-processing.

Gary Settles agreed that non-intrusive optical flow diagnostics was the way of the future. It

is an excellent idea to have a few centers where optical flow diagnosis, flow visualization, image processing and scientific visualization were emphasized along with the fluid mechanics. Maybe the sponsors at this meeting will get the idea. In the mean time, a major problem is that users of this expensive optical equipment do not communicate with each other in areas such as the difficulties in getting equipment to work satisfactorily, and the provision of software. More co-operation is needed; if you are interested in a new technique, then you need to visit the lab where it was developed, and the people there should document the method. This would help tremendously to pass the technology around.

Hesselink felt that a center would be within the funding capabilities of NSF or the DoD agencies. A lot of sharing of optical techniques is going on already, for their (Stanford) software can be obtained through FTP. Sharing can become a burden, as it takes resources to send out programs.

Steve Kline addressed several aspects of concern. He pointed out the significant progress in flow visualization techniques that has occurred in the past twenty years from basically manual methods to the computerized techniques today. The depth of understanding of a problem is often determined by the data information rate, for example, the jet-screach problem, requires full field information. He supported the idea of instrumentation centers since they would ensure a critical mass of people working and communicating in the one location. Instrumentation development should not be buried in some other project, which is done at present to get funding. A recommendation from this Workshop could well kick on the establishment of these centers.

Hesselink said that a center should not be isolated from fluid mechanics problems

Ron Adrian pointed out the limitation on DPIV is the data transfer rate. TV cameras run at about 10 MHz data transfer rate, which is equivalent to 30 frames per second at 512 x 512 bytes per frame. But 1/30th second is too long between exposures for many flows; to limit particle displacement to 1 mm at 10 m/sec requires 1/10th of a millisecond between exposures. One reason for using photographic techniques is that if you lay down two exposures in a time as short as 10 nanoseconds, then the equivalent data transfer rate is about 10^{17} bytes/sec. Photographs can be thought of as highly parallel recording devices. Each square mm contains about 512 x 512 pixels - about one TV camera frame - so a 5 in by 4 in photo contains 12500 simultaneous fields. DPIV is very useful for small, smooth velocity fields. Instantaneous fields with a wide range of scales will continue to require photographic recording until somebody makes pixel arrays of the order of 10 - 30,000 x 30,000, which would be comparable to a medium level photo.

Browand agreed that DPIV is best in liquid flows at low velocities.

Lex Smits emphasized the importance of relating instrumentation developments to particular problems.

Seymour Bogdonoff said this session is supposed to be about new techniques in turbulence research, but he couldn't see the link between the complex diagnostic tools now available and the engineering information you need about turbulent flows; the skin friction and heat transfer rate. What instrumentation will measure the skin friction and heat transfer comparably to these great steps in flow visualization? Whenever you do a flow field you must connect it to the surface if you are going to solve the problems of the future. Flow visualization has been around forever, but there are always questions of interpretation. The flow field should be related to the streamline pattern, you need to know where every

streamline goes.

Steve Kline felt that it is more important to know the vorticity field than the streamline pattern for turbulence. You won't get the time information from the streamlines.

Hesselink: We've looked at critical points and how you develop from the surface out. The advantage of quantitative flow visualization is that you can find the vortex core from helicity, and we have done that.

Seymour Bogdonoff: How do you tie measurements at $y^+ = 1$ to heat transfer at the surface. You need surface measurements to give weight to the flow field measurements.

Hesselink said that skin friction and heat transfer can be made with integrated sensors and the techniques can be combined with flow field visualizations.

Bill George said that flow visualization is our most powerful tool. For example, it can quickly tell if separation is occurring and removal of separation may solve a heat transfer problem. Regarding quantitative optical techniques, he said that in the late 1960's everyone thought the laser Doppler anemometer would be the solution to our inability to measure the fine structure of turbulence, but it turned out not to be the case. It took a lot of sophisticated and detailed analysis to find out why it couldn't. This analysis has yet to be done for any of these newer methods; there are signal processing problems, there are population changes in what is being measured and all those could sabotage the techniques. So before saying that these are the answer, let's recognize that a lot has to be done - a lot of fluid mechanics and a lot of signal processing. The ideas are clever and they may work. If so, they will be very welcome.

Hesselink said not everything had been done but a lot of work has been on signal processing and spatial resolution issues have been addressed. You cannot say a priori that a technique will solve a problem. If it looks promising and it's different, then it should be investigated. That is what basic research is all about.

SESSION 4

Educational Issues

Session Chairman: Hans Fernholz, Technische Universität Berlin

Session Recorder: David Wood, University of Newcastle, Australia

William K. George, State University of New York at Buffalo

I feel a little bit like Chuck Smith who introduced his talk the other day by saying, "I'm not sure why I'm here." I've thought of two possibilities, the first of which is that Lex Smits couldn't find anyone else who was willing to take on this topic (except for Ron Panton). Or the second (which is my favorite): He thought I knew something about "Graduate Education in Turbulence" because of my extensive involvement with it over the past two or so decades. My enthusiasm for this was tempered substantially when I saw the attendance list and realized that many of you have even more experience than I. Which leaves us with only the first possibility.

A proper graduate education?

In the past ten years or so I have had the opportunity to interview a considerable number of prospective faculty members who had just reached the end of their doctoral programs. From these experiences and the casual encounters at technical meeting with many other recent or near graduates, I have formed an opinion of present graduate education. What I fear represents a near-consensus of the educational community (the present audience excepted, perhaps) is:

- Take an absolute minimum number of formal courses.
- Hook onto an existing chain of research, preferably one well-funded.
- Make a modest, or even trivial, extension of the previous results.
- Quickly leave for a post-doctoral position, or some unsuspecting¹ industry or university.

Tony Perry addressed this in a somewhat different way earlier, but I'm sorry to say that the

implied indictments are the same.

We've heard quite a bit over the past decade (and at this meeting) of the technical needs of the nation to focus more on applied research. This national agenda appears to be based on the premise that somehow the failures of the past are due to improper directions in graduate education. Now I've thought about this a great deal, and I'm not sure I understand how we are supposed to "direct our students" toward applied research problems - especially if these problems are not fundamental in nature. First, I would argue that most research problems, especially in turbulence, have a strong applications link to begin with. Second, let me state my belief that a proper dissertation research will be fundamental, regardless of whether the topic is applied or more esoteric in its origin. Thus I do not understand the conflict which is presumed nor the remedial steps which are to be taken. What I suspect is being advocated is that we are being asked to deliberately teach students to think in a somewhat sloppy manner, to reason less critically, and to be less concerned about identifying and pursuing those things they don't understand - in short, to be satisfied by simply assembling the old ideas in an uncritical way to provide a quick fix for the problem of the day.

Now I have to confess that no matter how successful my own students may appear to be at these things, I really can't claim the credit. In fact, in my sixteen years at Buffalo and the prior six years at Penn State, I've found few incoming students to whom sloppy reasoning, lack of critical reasoning, and limited concern for fundamentals did not come naturally. My objective as a graduate educator has always been to change them to fundamental thinkers. That I have not contributed thereby to thwarting the national agenda is evidenced by the fact that a significant portion now work in industrial and national research laboratories on problems that are indisputably applied.

Also, let me make it clear that I have had very little success in directing my students toward any special kind of post-doctoral research, applied or otherwise. By the time they are finished with their graduate studies, they really aren't listening to me, or anyone else for that matter. In fact I've been accused of letting my students graduate when they were so independent that I couldn't stand to have them around anymore.

Now let me tell what I think a proper graduate education should be. The model I believe that is appropriate is that which has been used through the centuries to train specialists of all types; namely, that of the Master, his Journeymen, and the Apprentices. The Professor is, of course, the Master. The Post-doctoral and Research Fellows are the Journeymen, and the Graduate Students are the Apprentices. Literary writers and historians have documented for us numerous examples of abuses of this system, and perhaps some of them should be of concern for us as well. Apprenticeship is not slave labor. Nor is it purely an educational experience. Ideally the apprentice is learning about both the skills of his trade and about life in a broader context, and is at the same time performing useful service. This service has two purposes: To compensate for the expense of his sustenance, and to allow him to learn by doing. These can all be kept in balance only if the Master understands clearly that his objective must be to produce a graduate who has acquired the skills to function without his supervision.

In the trade apprentice programs it is usually obvious what the objectives of the apprenticeship are - to make shoes or hats, for example. What are the objectives of a graduate education? What are we trying to train the students in? While I will not concede that we are guilty of not orienting our students toward applied research (nor will I concede that it even makes sense to try), I will concede that we often do not have a clear picture of what we are trying to accomplish in a graduate program. And I suspect that this lack of a

clear objective may be in part responsible for the criticisms which have been directed towards us.

I suggest that a proper graduate education:

- * Trains students in the application of the scientific method.
- * Develops advanced skills in critical reasoning.
- * Demonstrates the excitement and satisfaction of finding solutions to unraveling the secrets of nature and the puzzles of man.
- * Builds the confidence necessary to tackle real problems.

Notice that once the objectives are understood, it matters little whether the particular research problems addressed are fundamental or applied - as long as they are consistent with meeting the objectives. There are engineering approximations which must be made because of problems encountered in the normal progress of the research, and unanticipated obstacles which require that the objectives scaled down. As John Lumley said the other day: "We promise to do a whole lot, knowing very well we can't do it (paraphrase)." Steve Kline spoke of technology-motivated research, to which John Lumley replied that we have always been doing that. I would agree - at least for many of us. We have all chosen research problems which reflect our own interests and our unique personalities. Since many of us come from engineering backgrounds, it is natural that our research problems - whether fundamental or applied - stem from the needs of technology. Some of us are experimentalists, others are theoreticians. Some of us have labs full of devices - probably because we get excited about machines and parts of machines. Others are content to sit at a computer terminal and generate pictures. While still others contribute and receive satisfaction with nothing more than pen and paper.

I argue that if a student is properly educated by the program into which he has entered, he can contribute in any environment in which he finds himself after graduation. The particular nature of the problem which he has investigated for his dissertation is irrelevant - whether applied or fundamental, whether experimental or computational or theoretical. A Doctor of Philosophy program is an exercise in learning to observe and think critically about the world around us. And once one has engaged in this, the effects on him are irreversible. Sadly, it is my suspicion that many graduate today having produced a piece of research, and having only been minimally influenced by it. They have acquired new skills - computing, measuring, etc. - but have not been transformed into the critical thinkers that constitute researchers. How much of the complaint that contemporary doctorates are not able to approach applied problems is really merely a symptom of this deeper problem?

At the risk of sounding immodest I can offer as example my own students whose research problem were of the most fundamental nature, and who were subjected to the rigorous and fundamental course structure described below. Of the ten Ph.D. students who have graduated over the past twelve years under my supervision, 40\% have ended up in universities, and the remainder have been distributed between industry and national laboratories². Of the ten, only three are at present actively working in turbulence research. The remainder have carved out new areas of interest for themselves on topics ranging from paper production to applied optics - topics which were in no way related to their dissertations.

What skills did these former students of turbulence bring then to these diverse subjects for investigation? The primary skill brought was the ability to learn about a new subject, and recognize quickly the fundamental aspects of it. It really didn't matter what they had studied in the past nor where their expertise lay. What did matter is that they had learned to learn about something new. Not just the quick shallow overview that enables one to speak glibly and for which so many settle, but they had learned to seek the kind of understanding that comes from taking a subject apart, from dissecting it into its basic elements and identifying those fundamental aspects which both enables an appreciation of what has been done and an identification of what needs to be done. Their success at applied problems far-removed from the fundamental turbulence studies of their graduate years is both a tribute to them and the program from which they sprang. Whatever its methods, the product was good because its objectives were clear: Not to publish the results of their research, nor even to get the next grant (although these are certainly worthy subordinate objectives), but first and foremost to change and refine them into critical researchers.

Formal Course Structure

The role and number of formal graduate courses is among the most hotly debated subjects of graduate education. One of the advantages of joining a university in its infancy has been the opportunity to formulate from scratch those programs and policies which will later become part of the tradition. I am fortunate, together with my colleagues, to have had this opportunity at Buffalo, and even after sixteen years I am quite pleased with the results of our efforts. To probably no one's surprise the graduate fluid mechanics program we developed bears a strong resemblance to the mechanics program I came through at the Johns Hopkins University in the 1960's. That program probably evolved in similar manner in the 1950's, drawing from the experiences of its gifted faculty at other successful universities, most notably Cal Tech.

Thus there is little new about what we do at UB. What does seem a bit unusual, however, is that we still do it in the 1990's. This is because there seems to have been a major roll-back in the availability of real graduate courses in the 1980's. This is perhaps attributable to increasing pressures on faculty from funded research and undergraduate education, and to the proliferation of PhD programs without the necessary concentration of faculty to offer a broad spectrum of fluid mechanics courses. Since neither of these problems is likely to vanish in the 1990's, our experience in dealing with them at UB may be of some value to others.

Our graduate program in Fluid Mechanics is essentially a two-tiered structure. The purpose of the first level courses is to rapidly bring the student to a point where he can begin to do serious research. The core of this first level is a two semester course in Fluid Mechanics (using Ron Panton's book incidentally). The course has a strong mathematical and physical content, and makes liberal use of the NSF Fluid Mechanics film series. This course bears the brunt of the responsibility for taking students from the undergraduate fact and skill oriented education from which most of them came, and "turning them on" to the critical reasoning necessary for research. This is not always a painless experience because of their diversity of backgrounds and personalities, and we have learned not to put too much weight on performance in it in our evaluation of a student's potential for further study. (In fact, one of my better students nearly failed it, but is now easily conversant in the special language and thought that is Fluid Mechanics). One semester courses in CFD, Turbulence and Experimental Methods as well as courses taught by other segments of the university (e.g. Applied Math, Heat Transfer, Turbomachinery, Mechanics, etc.) allow the student to pursue his own special interests in making up the remainder of this first year.

By the time a student has finished these tier-one courses (usually in the first academic year), it is expected that he will be fully immersed in his research problem. My colleagues and I differ on the relative merits of an MS thesis. My own preference is to by-pass it, and have students proceed directly for a PhD. The MS degree is offered as a consolation prize to those who for some reason must terminate their studies prematurely. My reasons for this are quite pragmatic and may be of some general interest. At the large state university that is UB, a disproportionate share of the students are first generation college students. While they understand why they are in graduate school, their parents (who may have been most supportive when they were undergraduates) often do not. The combination of home pressures, financial stress, and the usual tensions between advisor and student of bringing a thesis to final form makes them unusually susceptible at this time to the rather lucrative offers industry sets before them. The problem is exacerbated by the unscrupulous practices of recruiters who convince them that a PhD will be of no value - all the while hiring every imported PhD they can attract. After losing several good graduate students this way, I quickly learned to offer neither the frustration nor the opportunity - hence no MS.

Once the student becomes immersed in his research, my role as mentor changes. To this point my objective has been to focus him as quickly as possible on his research, while at the same time providing him with the tools to begin. It doesn't take the student long (usually) to figure out that this is where it is at - meaning: you don't finish until you get it right! Once this realization takes hold, the research problem becomes all-consuming and all other interests tend to be screened out. It is now my responsibility to counter this narrowing process by forcing the student to continue to be receptive to ideas over a broader spectrum. We accomplish this deficiency at UB by requiring (over and above departmental and university requirements) that the student take a sequence of advanced PhD level courses in fluid mechanics which are for the most part removed from his immediate research interests.

This PhD core consists of six courses which are offered on a three year rotation and includes subjects like stability and transition, viscous flows, potential flow, compressible flow and higher approximate methods. All of these courses are restricted to advanced students and a special effort is made to incorporate the latest research results by the faculty who teach them (after an overload and at personal sacrifice). (It is important to note that most of these courses do not correspond to areas of active research by the faculty involved in teaching them, but the responsibilities are nonetheless shared by all in the interests of a quality program.) Our overall objective is to enable the student to walk into any session of the APS/DFD meeting (or any other fluids meeting) and have a reasonable understanding of what is going on. In other words, we are attempting to graduate real fluid dynamicists in the classical sense.

Aside from its obvious purpose to broaden the student's horizons, there are a number of other benefits from our rather arduous requirements. First, the fact that all the PhD students in fluid mechanics are taking the same course (and usually only one) provides a common denominator for discussion among them. Since the details of one's dissertation are seldom of general interest, in the absence of this common stimulation, the discussion among students often degenerates to the latest football score. Thus these courses raise the intellectual level of the total graduate experience. Second, these courses remind the student that he is studying fluid mechanics, as fact many seem to forget when overwhelmed by the technical complexities of modern research. This reminder of purpose seems to help prevent (or at least diminish) the sense of isolation that develops at the darkest hour of one's research, and provides the perspective of belonging to a community of scholars, the fluid mechanics community. Finally, these in-depth courses make for a better researcher because they develop

understanding and confidence at a whole new level. And in this vein I might add that I view them to be even more important for experimentalists than theoreticians. (How many times have you seen experimentalists become glassy-eyed as they were flim-flammed by theoreticians they were not properly prepared to understand?).

Now the benefits to be gained from the program I have outlined do not come for free. In recent years as our fluids faculty has grown at UB, we have "bought" the luxury of teaching these courses with 5-10 students "on-load" by teaching larger sections in selected undergraduate courses. It is my contention that by creatively using audio-visual aids, teaching assistants with recitation sections (see below), and carefully prepared lectures that 160 students can be taught undergraduate fluid mechanics, heat transfer, etc. even more effectively than we can handle 40. (My colleague, Irving Shames, has done a spectacular job of this for decades, even without the recitation section.) The reason is that the nature of the material at this level lends itself to this approach. It matters not to the student whether he asks his question to a full professor or to a carefully coached graduate assistant - as long as he gets a cordial reception and an answer (in his chosen language) when he needs it. Most undergraduates in my experience would much prefer to meet a graduate student on their schedule than stand outside my office waiting for their turn. Regardless of the merits of large versus small (if 40 students is ever small!) classes, the important point is that without this compromise, there can be no PhD program! We have a constant quarrel with graduate program reviewers and administrators, who failing to understand our purposes and the compromises we have made, criticize our small graduate courses. To this point at least, we have successfully defended our choices by articulating our reasons.

Finally, it might be objected that these courses detract from the research and lengthen the students time in graduate school. I admit that this may be true. However, I would note that no one can do research all of the time, and I suspect most of the time spent on these courses is at the expense of alternative leisure activities. Regardless, what is the measure of a successful graduate program? Certainly not the time-frame over which research is produced, nor even the research itself! Rather, our focus should be on the quality of the product we deliver - the student himself! However important it may seem to be to publish quickly and keep our sponsors happy, if we let these determine our educational priorities we are letting the tail wag the dog. AT UB at least, the extra time (if any) appears to have been well worth the complications it presented.

Learning To Be Honest

The other day, someone (Rabi Mehta, I believe) remarked that graduate students should be taught to teach in graduate school. I agree, but perhaps my reasons are a bit more complicated than might be obvious, hence the title of this section. Specifically, I believe that graduate students should be required to stand before classes as recitation instructors, and I regularly require my own students to do so, regardless of their source of support. When I teach a course, they become my instructors. I usually subdivide the course into recitation sections of 20 or fewer students for which my graduate students are responsible. Now you may ask, why would I deliberately interfere with their research in this manner? The most obvious reason (but not the most important) is that their time is less valuable than mine. Also they can be more available to the undergraduates than I can usually be (see remark above). Neither of these is, however, the real reason I impose this requirement on my own students.

The real reason for this extra requirement is to teach them to be honest - before it is too

late. Too many times in my career I have encountered people with PhD's who simply could not admit their ignorance in public. How many times in your own experience have you seen a professor respond to a question by insulting the questioner, or by simply waving his hands and baffling his audience with nonsense? And why does this happen? I contend that in large measure it is because he believes that because he has a PhD, he MUST not admit that he does not know, or somehow his stature will be diminished. This is, of course, a complete misunderstanding of the meaning of the degree. A PhD is supposed to be an expert in identifying that which we do NOT know, and separating it clearly from that which we do KNOW and that which we only THINK we know. But most of us get trapped in the psychology of the lay world which views us as knowing everything, and therein lies the problem I am addressing.

If a person does not learn to absolutely honest to his questioners BEFORE he gets his PhD, it is unlikely that he will be able to adjust later. What better place to learn this than standing before a group of students only slightly less experienced than you. There is no benefit to not admitting that you do not know an answer, and all to be gained by answering that you will find out the truth and return with it. And because you are not yet Dr. so and so, neither you nor the students before you feel cheated. Telling the truth in this manner, and reaping its positive benefits becomes habit-forming, and these habits remain for a lifetime.

Thus why teach as PhD student? It makes for a better scholar who is confident in his own integrity. And there are, of course, the additional benefits of learning to be articulate and confident in public. And finally, it is an opportunity to learn in a whole new way. I don't think I ever really learned anything until I had to speak about it and defend my views about it in public. To some degree, at least, I suspect that is true of all of us.

Does a regular teaching requirement slow down the student's progress in his research? Again I ask, What is the objective of his graduate program? The objective is the student himself and his preparation for handling himself when he leaves. Thus, even if it does slow him down, the benefits are more than worth it if we properly keep our objectives in view.

The Role of the Professor

There is a constant tension in many universities between teaching and research. I believe this tension to arise in part, at least, from an inadequate (or even wrong) understanding about why we do research in the university. While it is true that we have obligations to society to apply our collective brainpower to their needs, and we also are driven by our own curiosity, I suggest that the primary justification for research in academia is that it is our teaching tool for these apprentices we wish to train. Just as we can not teach writing without writing materials, or reading without books, you cannot teach how to research without doing it. Thus from this perspective, research is as much a part of teaching as standing in front of a class and lecturing³.

Now if this is all so obvious, how did the tensions arise in the first place? My suspicion is that we the professors who supervise research students are responsible for most of the confusion. This is because that in our quest for fame and glory as researchers, we have forgotten what our mission was. I contend that the proper measure of a PROFESSOR's success as PROFESSOR is not his own research as measured by his grants and publications, nor even the publications resulting from that of the students under his supervision. The only real measure of his success as PROFESSOR is WHAT HIS STUDENTS ACCOMPLISH AFTER

THEY LEAVE HIS SUPERVISION.

Now this is probably hard medicine for most of us who fight for the next contract, promotion or raise by driving our students to produce. And in doing so we have been sucked into a system which rewards us for these activities - perhaps more because they make the administration or university look good in some survey than because they measure our contributions as educators. Yet in the final analysis, if we are to masquerade as educators, then we must be evaluated as such. And the only real measure of our success is the ability of our students to contribute independently after they leave us. If they graduate and are never heard from again, or are so destroyed emotionally or physically that they can not function, what does it really matter how many JFM papers were published from their dissertations?

I like to think that I am a good enough researcher that I could take a chimpanzee and produce research of sufficient quality to be published in JFM. (This should not be inferred to be an assessment of that or any other journal.) A much greater challenge for me is to take an insecure undergraduate whose horizons are bounded by the small world in which he has lived, and transform him into a confident scholar who believes himself to be among the best in the world at what he has chosen to do. Thus a big part of my task has to be to build not only his reasoning skills, but his confidence. However gratifying it may be to me for him to think I am brilliant, until he thinks he is as smart as I am, I have failed.

And how can this great ego massaging be accomplished?

First, by proving to him through an arduous program that he is truly competent. (We've already discussed some ideas for this above.)

Second, by allowing him the freedom to discover things on his own. (In this day of high pressure funded research, it is hard to stand back and take the heat from impatient sponsors while we allow our students to find their own way, but that is exactly what we must do if we are to accomplish our objectives as educators. One of the greatest challenges I face with students is to not yield to these pressures and intervene too early.)

Third, by exposing them early and often to the rest of the scientific community. (Frequent appearances and presentations at scientific meetings are a must if they are to come to believe in their own worth and have a proper perspective on it. Again, these opportunities come at some cost to the professor who would like to be presenting the work himself and probably would do a better job.)

These guidelines offer a considerable challenge, especially to those who are just beginning their careers in academia, since they must be evaluated before they can be judged by the accomplishments of their students. My advice to them: Try to develop research ideas of your own, independent from the problems on which your students are working. That way you are not inextricably linked to their success or failures, at least on a daily basis. And whatever the pressures, never forget why you are in the professor business in the first place: STUDENTS, STUDENTS, AND STUDENTS. Ultimately you will be measured by what they do when they leave you.

Final Word

The comments and ideas expressed above are a highly personal view of graduate education in

fluid mechanics and turbulence. Certainly I am not naive enough to believe that my ideas are the only ones which will work. History certainly would dispute that. Also I'm not really old enough (45) to even be able to say with confidence that these ideas have worked for me. However, I suspect that most of what I have said has been gleaned from my own experiences, observations, and particularly my associations. To the extent that this is true, then it is reflective of traditions begun by von Karman, Prandtl and Taylor, not to mention their highly successful students and students of students, some my mentors. Thus if the objective of a graduate program is to produce independent thinkers who continue the tradition, and if I have correctly divined why these men have left us such a legacy, then I am on very firm ground indeed. Regardless, I thank for the opportunity to refine my own thinking and share it with you.

Footnotes:

¹Unsuspecting: in the sense that the employer probably expects his new employee to function independently with a reasonable perspective.

²Note that several have moved back and forth, but the distribution has remained nearly constant. It is interesting that these are approximately the same ratios for the attendance at national research meetings, like the APS/DFD.

³In my department at UB, we actually compute workloads by assigning a classroom equivalency to the research supervision of graduate students.

Ron Panton (University of Texas at Austin):

Brian Cantwell told of how his mother finally figured out what he did. She decided that "fluid mechanics" was the study of chaos. I had a similar identity crisis when I was young and came back to a family reunion at my cousin's house. The topic came up; "Well, what in the world are you doing with all this higher education you're undertaking?" It came out that I was in the area of Fluid Mechanics. My cousin thought awhile and finally said "Does that mean that you can fix the toilet?" So from chaos to water-closets fluid mechanics spans the spectrum of activities. I knew I'd follow Bill George and I thought I would call him up and talk it over, and divide this up, and then I thought that this might be interesting if we just both went at it not knowing what the other was going to do. George always has an unique and interesting view point so I don't think this is going to be too repetitious.

The first view graph is a look at the audience that we have for fluid mechanics graduate education, the various disciplines that they come from, and what their ultimate interest is in fluid mechanics. I have a lot of aerospace students in my courses, occasionally chemical engineers will come through, civil engineers - we have on our campus a big water resources group. In mechanical engineering, fluid mechanics is an educational part of thermal engineering. Petroleum people occasionally come over to my course. They are interested in "production," all the processes for drilling wells and all the processes of transporting fluids in pipelines, and "reservoir dynamics." Reservoir dynamics is what they really specialize in in their fluid mechanics. We don't have the Meteorology Department anymore, so I put those in just at the end. In summary, that's where the people come from who want or need a fluid mechanics background. Our subject "Turbulence" is an integral part of a proper fluid mechanics education.

I started listing on the next figure where these people go, and what types of jobs they end up in. I put both the theory and applied columns here to make sure that those things are mixed up together. I think that engineering students should develop an attitude that they know what the purpose of their work is. Of course, research is the job that we do. Research people end up as University teachers, they end up in government labs, industrial labs, and occasionally we have a university lab. For example, we have an acoustics lab that has been on our campus since World War II. We also have a new hyper-velocity lab. These research people are professionals in those laboratories.

In addition to the research into the generation of knowledge, there is the practice of engineering. Our students, although they participate in a PhD program or a MS program, a lot of them will end up in here - engineering practice of design and development.

The university labs do practical engineering to a certain extent, and we also have the other government labs that might be interested in engineering practice. Places like Wright-Patterson AFB. Then I didn't want to forget that there are a lot of times when engineers as they go through their careers they gravitate out, become less technical, they work to the production and modification and non-technical things that are involved in our industrial society. Furthermore, to a large degree, our graduate students end up in

management. They end up in coordinating positions, I have had students who are working at Sandia and they're heavily involved in coordinating projects. I think we, that is former turbulence students, have gone all the way. There was a university president that came from the fluid mechanics community. The person that is presently the vice president for air force projects in the Rand Corporation, once counted the spacing between streaks in the sublayer. So the actual career positions that we have, we have to take a broad look at, because I think these people are moving through our society into varied and numerous positions.

From this viewpoint our specific turbulence education is important only as a typical experience in general graduate education. Bill George said something similar previously. Tony Perry, as always at the same time humorous and profound, gave a litany of the "people" skills and attitudes graduate students must have to make a research project a success. For those who move into management, the development of work attitudes and ability to deal with people are maybe more important than some of our specific scientific things.

The title of the next view graph is "professional development." This segment is supposed to deal with the development of people who are faculty members as well as people who go into management. I'd like to trace the breakdown of how that might be categorized in different ways. I started off with saying there are three levels of technical education: the Masters degree, the PhD degree, and there is sometimes something after the PhD. This is a person moving into their first professional position someplace, maybe they have had a post doc or visiting position and then they go to a first professional situation. Graduate education contains a lot of theory courses; those are our favorites, of course. The ultimate purpose of research is to produce theory or techniques. We give a lot of theory to MS students. That's where they learn what's really going on. In a concentrated year or two of effort they probably increase by 100% their technical understanding. PhDs also have some of that start. There are also applied courses in the curriculum. If you recognize that the engineer that you produce is very possibly going to be designing helicopter blades, then he is going to want to know something about design methods for turbulent boundary layers and the practical codes that are used in those processes. So we have an obligation to teach what I think are applied courses. I'll show you an example of one, a little bit later here. The next item is scholarship, individual learning, the ability to learn on your own, as George was talking about. I don't see much of that happening at the MS level. At the PhD level though scholarship is a necessary activity. We have been working in this technical area for a long time and we know all the background terminology, vocabulary and what the critical issues are. A new student on a project has a lot of scholarship to do in order to come up to speed. That's usually an individual thing. If he is a really interested student, will try to do scholarship in filling out his knowledge of the theory courses. So scholarship continues on, its a major activity for a young person and probably he should continue on for maybe the rest of their productive life, really.

The next item is research projects--projects that have been planned by another person. This is where the student develops his phone skills, his don't-ruffle-the-machinist ability, the ability to deal effectively with bureaucracy. He plans projects orders, figures out sizes, specifications, and so on. Our PhDs have of course a lot of that activity because universities do not generally supply staff people to do that work. I find that our system of funding is such that a person gets a PhD without really initiating the project, conceiving the plan, and defining what the goals are. Those are the important intellectual exercises that have already been done for him by the person that got the grant. I think that if we didn't have to get that grant, we could make our PhDs do better in terms of their ability to define projects and figure out important ways to solve problems. So I put this little square in here because I think there are a lot of PhDs who come up who are weak in that area. The item is

"visiting positions." There are several types of visiting positions around and they are useful educational experiences. I'm thinking not only of post doc positions, but also NASA, AFOSR, and ONR programs where faculty members, particularly young faculty members, are taken into government labs for the summer. Those are very very valuable in initiating new faculty to the people who are working in applied projects. I think it changes their attitude quite a bit and I've always been in favor of faculty knowing the spectrum of activities needed in engineering. Moreover a person on one of these summer programs get to know the people at NASA. Those programs have been around for a long long time and I think they have been very useful. We once tried to do a little bit of industrial internship for our students, but it was not something industry wanted to put any resources into. The visiting positions are an important part of education. In terms of initiating research projects we have always had the NSF research initiation grant procedure. It has been there for many, many years. To be able to get a little bit of money to start a project of work. So for a young PhD just graduating taking up their first position in researcher in an academic university, I think that there is quite a bit of help available. I think that our system works pretty well. The only criticism I have is that we over-define PhD. projects at the start.

Well, let's see, I guess I promised to show you what I thought was a good applied course. Turbulence is the pervading fluid mechanics problem and modeling of turbulence is a thing that all industry people want. Here is a sample of a syllabus for "Modeling of Turbulent Flows," a course that we teach at Texas (Figure 3). You can see how it proceeds to higher and higher levels. But, it's a very practical course, very non-theoretical I guess you might say.

I've got one more slide here. It's a typical list of course work for students doing turbulent flow research project (Figure 4). These are the courses we might recommend that a student take. He would take incompressible flow theory course, incompressible flow applications course, that means aerodynamics and boundary layer theory essentially. Then, he would take a course in the structure of turbulence shear flows. This similar to the Tennekes and Lumley in scope. The turbulence modeling course that I just flipped through would be recommended. An experimental methods course is also taken; usually at the first semester. Experimental methods would be not only for turbulence but for our entire thermal science area including temperature measurements. It would have laser, LIF, LDV, hot wire; things like that. Then there are additional broadening courses that are necessary because this person is supposed to go out and eventually become the president of the University or he is supposed to become director of NASA or some similar thing. So he needs to know a little compressible flow, hypersonics, and things like that. That's pretty much the end.

I had down here a two million dollar project I want to advocate, people are making proposals, so I thought I might as well make a proposal too. It has been some years since the National Committee for Fluid Mechanics Films has operated and a couple of the authors are with us today. Those things are put out in movies. If you bring a movie camera out, a student will wonder it is. It's an antique as far as they are concerned. Now there is a lot of good information in those films, but students just shudder when they see an old film. They think the material is out of date. It lacks charisma. It's not in the format which they need, it's not in color, well most of them aren't in color. So I think that a good, very expensive project would be for this nation to improve its fluid mechanics education by doing another set of fluid mechanics films. We know a bit more now, we have got a lot more flow visualization methods that are whippy and very interesting, we have computer techniques, computer visualization techniques that are very dynamic, and so on. As long as your building a high Reynolds number facility for a billion dollars, we should spend two million dollars on educational video.

Audience for Fluid Mechanics

Aerospace:	Aerodynamics Combustion Material Processing
Chemical:	Processing Rheology
Civil:	Hydrology Environmental Water Resources
Mechanical:	Thermal Engineering Heat Transfer Combustion Biomedical
Petroleum:	Production Transportation Reservoir Engineering
Meteorology:	Research

Career Positions

Research (Theory/Applied)	University Teachers Government Labs Industrial Labs University Labs
Engineering Practice (Design Development)	Government Labs Industrial Labs University Labs
Engineering Services (Production, modification, sales, service)	Industry
Management (Direction, oversight, coordination)	Government Industry Universities

Professional Development

	MS	PhD	Post PhD
Theory Courses	X	X	
Applied Courses	X	X	
Scholarship		X	X
Conduct Projects	X	X	
Visiting Positions			X
Initiate Projects		X	X

Course Work for Turbulence Project Student

Incompressible Flow Theory

Incompressible Flow Theory Applications

Structure of Turbulent Shear Flows

Turbulence Modeling

Experimental Methods

Additional Courses:

Math

Computer methods in fluids

Compressible Flow

Hypersonic Flow

Combustion

Heat Transfer

Discussion:

Don Rockwell discussed experiments that have had a big influence, such as the discovery of coherent structures in free shear flows, chaos and Rayleigh-Bernard convection, and outside fluid mechanics, high temperature super-conductivity. These were all inexpensive experiments. What was it about the investigators that allowed them to gain physical insight that provided a turning point for their particular specialization? I do not understand the "special something" but it is of great concern and is perhaps summed up by the title of the overhead: Will the graduate student of the next century be a collector of data or a manipulator of flow physics?

"We need data collectors but to provide impetus for further activity in turbulence research we need these important experiments from time to time. For me the issue of central importance for either laboratory or numerical experimentalists is the development of the ability to approach new, open-ended situations in a physically imaginative fashion consistent with the laws of physics. That sounds simple, but those of us who are past students will realize it is quite difficult. We admit people to graduate school on the basis of grades. They think that they are doing well if they get good grades in their courses. But the reality is that we want them to make original contributions to research. I don't think students realize that early enough. The "desired approaches" we would like to see in our students are:

- assessment of theoretical principles and development of an analytical model,
- comprehension of the factors that influence an experiment to be undertaken,
- formulation of experimental approaches,
- deciding which one to use first,
- execution of the experiment. Here the student runs into a highly interactive situation. During the experiment he tries to assess interactively which route to take. He must learn to creatively take risks. These risks may be unrelated to the initial justification for the experiment as he must sometimes try things that are off the beaten track.
- tie together the theory and experiment.

How do we develop these capabilities in a student? I don't have an answer, but it is crucial for the future health of turbulence. We need this ability to approach open-ended situations. In more detail, my concerns are:

1. I think that in a cultural sense, our society is becoming less intellectually curious. There will be losses in the creative processes of students we admit to graduate schools.
2. I am concerned about the emphasis on computers. I strongly advocate their use in all the senses addressed by this workshop but they must not detract from the student's ability

to approach a situation and get physical insight.

3. There is a shortage of visible laboratory role models when compared to computer scientists and entrepreneurs. Students don't see successful experimentalists as they go through the system.

Is there any way to curb these dangers? I don't have concrete answers, but we might consider:

1. Try to do more open-ended problem solving in courses.
2. Have a graduate course entitled "Experimental Situations and Alternatives" which would discuss real experiments and alternatives and inculcate open-ended thinking.
3. We should test student's ability to approach open-ended situations in their PhD qualifying or preliminary exams."

Bert Hesselink agreed with the point about not seeing innovative ability in students. He has a different perspective as he did his undergraduate training in Holland. There was no homework assignments in the first four years, and there was no need to go to class, only to pass the exams. While this is not an efficient way to educate a lot of students, if you were interested in a topic then you could spend time learning about it without pressure. At Stanford, students get upset at having to do open-ended problems because they have lots of other work - there is too much pressure. In some sense we have over-regulated the learning process. We are making these people into technicians who can solve problems but don't necessarily understand the fundamentals. He has tried in his courses to give problems with latitude and has found that this develops the abilities of the most successful students.

Another important issue which has not been raised is the small number of women either in the profession or in graduate school. We need to improve their representation.

Dennis Bushnell said that there is generally no class time devoted to study the process of coming up with innovative ways of doing things. There are textbooks devoted to teaching creativity. We have had some success at Langley in concentrating on that. We have been able to show that creativity is not something you are born with - it is something that can be taught and you can get more out of people if you teach them how to think that way. It is a well-studied, well-known process - it is not a mysterious thing that you either have or don't have.

Steve Kline estimated that it would cost around \$10m to remake the fluid mechanics films. While some additions may be valuable, he did not believe that the series should be redone. He pointed out that they are available on video. The use of black and white was purposeful as it improves the contrast. We (at Stanford) teach creativity in the Design division and have had in the Thermosciences division an experimental methods course that Bob Moffatt has been teaching for many years.

Karman Ghia said his experience of freshmen and sophomores is that they don't want open-ended problems, but this changes as you get to senior students. One of the reasons is probably the Design sequence that we must have for accreditation where creative ability is required. Once you start publicizing the prizes and the students know that they can get awards for their work they become motivated, they work hard and they can be very creative. Recognition is important.

Tim Wei said that the discussion was generic to graduate school. He asked whether we believe that experimental turbulence research is important enough to produce more PhDs. If so, then how do we make it attractive for students and junior faculty members. At the moment it is very difficult being a junior faculty member. We have to convince students that it is worth their while to go through a PhD program.

Bill George told of a Newsweek article he wrote in 1984 in response to the report of the President's Commission on Education. The article was never published, but the point was that education has ceased to be fun and we have squeezed out the creativity. If we do not make it fun, then talking about creativity is pointless. We have to stop using so much rote-learning and piling on homework problems. The challenge of graduate school is to turn this around.

Ian Castro spoke as an outsider. Students in Great Britain are not examined on courses as part of their PhD studies. How do we choose students who are creative and obsessive about their research, because he believes creativity often goes with obsession. Good research needs periods of obsession for creativity to come out. Some students never reach that stage. How do you face the problem of students who don't have it as at the moment they can still get their PhD. Another problem is that mathematical ability is declining. Even Cambridge is now considering toning down the quality of its math syllabus. This shows that problems occur much earlier in the education system than University.

Brian Cantwell agreed with the need for periods of obsession and said that research centers can contribute here. For example, the summer sessions of the Center for Turbulence Research have been very intensive, 13 - 15 hours per day for a month, and students benefited enormously from this.

Israel Wygnanski spoke also partly as an outsider, having studied in Canada. He said that many students in the US are spoon fed. For example, most courses in Canada are completed before the MS. In many US universities, a thesis is usually not required for an MS degree and even in a PhD program, courses are usually taken up till the last year. This fragments student's thinking about research and is detrimental to creativity.

Anatol Roshko replied on behalf of the "spoon feeders". Caltech has an MS based on coursework which is meant to sort and identify those who could go on to the PhD. In addition there is an experimental course which gives a chance to detect creativity. Coursework is important as it gives exposure to a large number of subjects, not just those that are the focus of the thesis. This makes for a complete scholar, which may not occur if the focus is just on the thesis

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 END OF DISCUSSION. WHAT FOLLOWS IS CONTINUATION OF DISCUSSION OF PREVIOUS
 SESSION WHICH WAS NOT RECORDED.

Ron Adrian did not get the chance to present his conclusions at the Langley workshop on boundary layer structure, so he did so here. There are many techniques that are ready for application in turbulence to improve our understanding, such as planar laser-induced fluorescence and PIV, one form of which was described by Fred Browand. An important issue is spatial resolution which is a significant challenge. It is interesting that for 25 years we have been content to use 1 mm hot wires and 1 mm long LDV probe volumes but we have suddenly decided that we must get smaller. Spatial resolution can probably be improved by a

factor of about ten, and this could reduce the size of a national facility.

He made two further comments about spatial resolution. The present PIV resolution of low Reynolds number channel flows is comparable to that achieved in direct numerical simulation (DNS). Numerical differentiation can be done with a resolution of about 500 microns. Without looking at the whole channel, this can be reduced to 100 microns. He showed color-coded instantaneous vorticity contours derived from PIV. The patterns are virtually identical to those achieved in DNS in both scale and shape, so the differentiation has not resulted in just noise.

What about high Reynolds numbers with its large dynamic range of scales? Probably it is not necessary to measure over all scales, as one could do experimentally something like large eddy simulation. He showed an example from measurements in an engine cylinder and suggested that generally, the decomposition into large eddy and sub-grid scales is preferable to conventional Reynolds averaging.

Tony Perry asked about particles that don't remain in the measurement plane and the resulting bias.

Adrian replied that the plane was 0.5 mm and particles move less than 200 microns between light pulses. Even if a particle leaves the plane, there is typically 10 - 20 particles left in each interrogation location. With a high particle density, the bias is a small fraction of the velocity variation which itself is small.

SESSION 5

Strategies for Effective Turbulence Research in a Changing Environment

Session Chairman: David Walker, Lehigh University

Session Recorder: Eric Spina, Syracuse University

Doyle Knight (Rutgers University):

I was asked to report back on yesterday's small exercise. Just as a word of review: the objective of this simple little exercise was to enumerate several major areas of turbulence research by asking each person to list at least one or more. The objective to find in the list the most important area of research.

Statistically, 38 people gave a response and 34 of those actually identified topics. I have made some attempt to try to categorize these in some fashion and whatever is evident in the categorization is my own selection. I have tried to be as objective as possible but that is never totally possible.

I have basically listed the topics in alphabetical order (I apologize for the quality of the viewgraphs that you see, but there is a hardcopy circulating around that you all need to get a copy of), and used some minimal judgement as to when two responses appeared to be suggesting the same topic. There is no attempt in this list to prioritize. There is no attempt in this list to try to indicate which is more important or less important. It is obvious, I think, that the list reflects the ages of the people here. You can draw your own conclusions as to whether the numerical response has anything to do with the importance of the problem. What I have shown in the right-hand column is the # of responses that indicated that particular topic. You will notice that it sums to more than thirty-four because some people listed more than one.

There are two pages. The responses, if you look from say, simple statistical interests as to what areas were mentioned the most number of times, alphabetically: atmospheric kinetics, which is weather control, global and local weather prediction. Coherent structures were mentioned as well in various sub-categories. One that appeared frequently in addition to combustion was complex flows, that is, the effects of extra strain rates, three-dimensional flows, interaction flows, effects of chemical reactions. Compressible turbulence, drag reduction, and finally on the second page, separated flows (this includes separated boundary layers, massively separated flows, stalled airfoils, control of separated flows), turbulence modelling - basically meaning the need to develop better turbulence models, more accurate turbulence models, and apply better predictions or postdiction of experimentally-observable

quantities.

Turbulence, kinetics and energetics is the best possible title I could think of for a whole host of topics which included such issues as mechanisms of production, of turbulence near walls, how turbulent energy balance is maintained in mixing layers in the presence of production and dissipation, kinematic and dynamic relationships between fine scales and large scales, spectral and spatial scales, and so forth. Finally a few had mentioned the issues of vorticity and vortex flow control.

James M. McMichael
Program Manager, Aerospace Sciences Directorate
Air Force Office of Scientific Research

Introduction

There are two basic themes that bear directly on the long-term issue of strategic planning for turbulence research and deserve serious thought and discussion. The first is that future support for basic fluid mechanics research in general, and for turbulence research in particular, may well hinge on our ability to strengthen interactions between the basic and applied research communities in fluid mechanics. There is a concurrent need to improve the attitude of each community toward the other. The second theme is that the continued vitality and progress of turbulence research might benefit from the development of a community-wide strategic view of the various issues that the turbulence community will be facing as the environment around it continues to change. With an uncertain future of tighter research budgets some very difficult choices may have to be made. By what mechanisms will the research community advocate its position, voice its concerns and provide input to the decision making processes?

I would like to start with a historical note. In 1944, the Commanding General of the Army Air Forces, Hap Arnold, asked Theodore von Karman to organize and conduct a study and develop recommendations on future directions for Air Force research and development. He was asked to set aside concern for the present war, namely WW II, and to look far into the future to recommend what the Air Force ought to be doing in the way of research and development. Arnold said in his letter to von Karman, "The security of the United States rests on the developments instituted by our educational and professional scientists." When the report was presented a year later, von Karman remarked in his cover letter that "research problems should be considered in relation to the functions of the Air Force rather than as isolated scientific problems." He also said, "The men in charge of future Air Forces should always remember that problems never have final or universal solutions and only a constant inquisitive attitude towards science and a ceaseless and swift adaptation to new developments can maintain the security of this nation."

Von Karman was telling us that basic research needs to be sustained with a long-term view, and, at the same time, the results of basic research need to be transitioned as rapidly as possible to new applications. He was stressing the need for a balance between the basic and applied points of view. Perhaps one way to restate this is that we cannot nourish every basic research avenue continuously, forever, in isolation from its eventual application. Nor can we solve every practical engineering problem with current technology, immediately, today, or even in time for the next quarterly report. Industry frequently adopts this latter view - looking for instant solutions to problems that are, in fact, very difficult. And because of their complexity, these problems are too often regarded as unattractive by the basic researcher who would rather work on problems which have been sufficiently simplified to be tractable. Perhaps it is difficult to appreciate that intellectual stimulation can also come

from progress on complex, "real world" problems. The scientific community also needs to realize that there are just not enough resources to pursue every scientifically attractive idea without regard to its potential relevance to the needs of today's society.

Von Karman's idea is also reflected in the basic mission of AFOSR, namely, to support and stimulate basic research relevant to future Air Force mission requirements (Fig. 1). While we need to be relevant, keeping the practical applications of basic research in mind, we also have a clear charter to provide a strong scientific base of fundamental knowledge. While many of us in the research community may tend to give primary consideration to the latter statement, we are also strongly encouraged to direct our thinking towards the relevance of the research to Air Force applications. This is really nothing new, we in the mission agencies have always structured our programs around relevant applications. But there is perhaps a diminishing appreciation of this in the research community. Ironically, few, if any, other areas of physics and engineering are as easily identified with so many real world applications as turbulence.

It also seems fair to argue that, if the field ought to be supported with public funds, you ought to be able to tell the public what benefits they can expect. This is why I think it is important to improve our communication of the value of basic research to the applied research community. If we cannot communicate this to the practicing engineer, how can we hope to communicate it to the general public or to the decision makers in Washington? Continued successful advocacy for turbulence research will depend on clearer communication with those developing future technological applications.

Structuring Basic Turbulence Research for Technological Relevance

Within this context, the following is a brief outline of the Air Force basic research program in Turbulence Structure and Control, starting with the question of Air Force relevance. A partial list of broad technology goals for our turbulence program can be grouped under general areas such as mixing, heat transfer, and perhaps drag reduction (Fig. 2). These technology areas can be thought of generically as the prediction and control of turbulent transport processes. Prediction and control are the main avenues for transitioning basic knowledge about turbulence to technological applications, and they constitute the principal technological thrust of our program. These technology goals can also be stated in very explicit terms. For example, our efforts in the area of turbulent heat transfer, if successful, will contribute to a coordinated technology program between the Air Force and NASA called the Integrated High-Performance Turbine Engine Technology (IHPTET) program, by providing more reliable prediction of heat transfer on gas turbine blades. We think we can do this on a time scale that will allow these improvements to help the IHPTET program meet its goal of doubling the thrust to weight ratio of turbine engines.

How do we go about structuring basic research programs to be responsive to these technological requirements? Our basic strategy is to support a sufficiently broad spectrum of research efforts to allow us to continually foster long-range advances in basic understanding, while also exploring avenues for transitioning that understanding to applications through new concepts for the prediction and control of turbulence.

The backbone of our program is directed toward improving our fundamental understanding of turbulence physics (Fig. 3). We are trying to produce new knowledge and better understanding of turbulence, and, at the same time, stimulate the development of new concepts and ideas, both scientific and technological. The reason that we seek to understand the physics of

turbulence is to be able to predict and control it, and to model turbulence for real world applications.

Our approach to research on turbulence physics embraces theory, experiment, and computation. The latter area is making great strides, bolstered by the rich (spatial) detail of new information that numerical simulations can provide. Detailed analysis of the new information available from numerical simulations complements our experimental results and seems to be driving some new theoretical ideas as well. These advances, in turn, will stimulate new directions in experimental turbulence research, which will demand greatly improved experimental diagnostics in the future.

Reliable experimental turbulence research is painstakingly difficult, and may become even more difficult if it is to successfully meet the challenge to develop this greatly expanded diagnostics capability, as must be done to address contemporary issues now being raised primarily by recent advances in numerical simulation. Obviously, technologically relevant flows tend to be higher in Reynolds number than typically available in university laboratories, and they are likely to remain well beyond the capability of our computational tools for a long time to come. This underscores the continuing need for contemporary experimental turbulence research, for research at higher Reynolds numbers, for new ideas, and especially for new experimental diagnostics capabilities.

It is ironic that technological advances in a number of fields like micro-electronics and optics now offer such tremendous potential for diagnostic advances in fluid mechanics research, and yet arguments for new resources to support the application of these technologies to basic fluid mechanics research are sometimes viewed as less attractive than arguments to extend the enabling technologies themselves.

I believe that we also need to give much more attention to the analysis of our results, particularly in experimental turbulence research. We probably spend 90 to 95% of our time with the acquisition of turbulence data, and far less, probably 5 to 10%, actually analyzing what the data means. This problem may be mostly cultural, that is, largely due to the paper counting algorithm for tenure decisions.

Allow me to take this slight digression just one step further. This tenure mechanism and other institutional peculiarities could radically change over the next decade. Roughly half of today's engineering faculty will retire in this time frame. How will this affect turbulence research, and what voice will this community have in meeting the challenges brought about by such changes?

The prediction of technologically relevant turbulent flows will require some form of turbulence modeling for the foreseeable future. What we mean by modeling is the representation of the net effects of complexity by a sufficiently simple model - a sufficiently simple idea - that available analytical and computational tools can usefully predict the net response of the complex system (Fig. 4). The level at which this complexity must be modeled depends on what you want to predict. It is not always necessary to explicitly capture complex structural or dynamical detail if you have a model that will adequately capture the net effects of that structural or dynamical detail. We also need to broaden our goals for turbulence modeling. For example, computational fluid dynamicists, using turbulence models to help compute flow problems in industry, should probably look beyond the popular application of these methods to parameter variation studies. I would suggest that more useful models should be capable of allowing us to predict the response of turbulent flows to external disturbances and especially the response to control inputs. Advances in large eddy

simulation approaches will also allow us to address questions about the physics of turbulence at higher Reynolds numbers.

At the interdisciplinary frontier of flow control, our central theme is the exploration of control theory for application to fluid mechanics (Fig. 5). Distributed parameter control theory (i.e. control of partial rather than ordinary differential equations) is a basic research area in its own right. In fluid mechanics, we are appealing to control theory to guide us in our experimental research, decreasing our present total reliance on intuitive or ad hoc control strategies. Only in this way can we really learn what is possible, in principle, in the way of flow control. One aspect that is not recognized at all by turbulence control empiricists is that a controlled flow can have entirely different stability characteristics than an uncontrolled flow in the same configuration. It is, in fact, an entirely different flow from the so-called "natural flow".

I have already touched on the need for expanded quantitative visualization and analysis capabilities. I consider this one of the most hopeful areas in experimental turbulence research. This includes the development of new diagnostic tools as well as methods of analysis. We are seeding a small amount of work in this area, but it needs a significant funding initiative to truly provide experimental access to the kind of 3-D spatial detail now available from direct numerical simulation of simple flows at moderate Reynolds numbers.

Current Funding Trends and Strategic Challenges for Turbulence Research

I want to turn now to the matter of funding for turbulence research, and offer some related comments on technological innovation and the role of program managers.

It is interesting to note that the overall 6.1 budget of DOD tripled in the two years following the launch of Sputnik in 1957. Perhaps strategic planning of basic research will never have quite the same impact as a sudden technological shortfall.

Nonetheless, it is important to consider a number of current strategic issues. Since 1986, we have seen a slow and non-precipitous decline in overall basic research funding. The AFOSR fluid mechanics program has seen a similar reduction of available resources, despite our successful advocacy of a number of research initiatives (Fig. 6). If you look at the overall funding trend composed of three major areas: our core program, the University Research Initiative program, and the discontinued University Research Instrumentation program, it is apparent that we have seen an erosion of roughly 20 percent since 1987. Over that same period of time there has also been some erosion due to inflation, something on the order of 20%. Conservatively, the net effect is that we have lost one-third of our total fluid mechanics research over the last five years. While I share your concern over this more recent downturn, I still feel that there are much more important overall strategic issues which should command your attention.

I should also point out that basic research is buried within a budget line called Science and Technology and this is all that shows up at the Congressional level. Fluid mechanics is deeply buried in the budget and fluid mechanics is certainly not visible at the Congressional level. While fluid mechanics is an important element in many national issues, including defense, energy, and environment, it is not a national issue per se.

There is a general perception that science has a certain fixed relationship to the rest of technology development, namely, that it is the first step in the sequential development of any

new technology. The overall DOD research and development program is structured in this same linear fashion with a systematic progression from 6.1 to 6.2 and so on. The common view is that technology begins with science and progresses linearly all the way to systems development. This simplistic view, unfortunately, reinforces the tendency for basic researchers to report their results in archival publications with little additional concern for transitioning new knowledge to useful application. It also fosters the notion that new scientific ideas come only from past and present scientific achievements.

But the process of technological innovation is much more complex with feedback and linkage between all stages of the innovation process. Professor Stephen Kline at Stanford has developed a nonlinear "chain link" model of this process which gives us a much richer context for understanding the process of technological innovation. The interface between science and technology spreads across the full spectrum of research and development programs. Furthermore, I contend that technology driven science can also be intellectually stimulating. I would like to appeal to the community to take a closer look at the processes of technological innovation. I believe this will help us deal more effectively with the need to communicate more effectively the technological relevance of our basic research programs. There is certainly no reason to avoid technology driven science just because something useful might come out of it, even in the near term.

What is the role of program managers in the process of innovation? Within our own agencies, one of our functions is occasionally said to be to "select and manage the knowledge suppliers." Knowledge suppliers, presumably, are you. This has the connotation that we manage people. I don't really think of our function in quite that way. We really manage programs, not people. One of our roles, in my view, is to serve as one of many interfaces between the research community and the applied world, and to serve as an interface between the research community and the management levels within our respective agencies. In part, our role is to facilitate some of the linkages in the innovation process. This requires a very different set of skills than those required to perform the research itself. Nonetheless, I believe that the research community should view us as a part of that community, with something to contribute to the research process, just as I believe that management should view us as part of the management team with something to contribute to the management process.

The Changing Environment and the Need for Wider Collaboration

There are significant changes taking place in the world around this community (Fig.7). For example, it seems likely that DOD budgets will shrink in the years ahead. There is talk about merging DOD laboratories. There has even been talk about merging the basic research agencies. Other changes include global socio-economic and political changes. All of these may have a significant effect on this research community as time goes on. The technical environment is changing as well. For example, in this community we are talking about wider parameter ranges, greater complexity, and non-canonical flows. Contemporary research issues are becoming more and more complex, and the instrumentation required to address them is becoming more complex and more expensive. These considerations drive the cost of research up and yet, at best, research funding seems to be a zero-sum game.

One recent technical change is illustrated by the Center for Turbulence Research (CTR). This center has played an important role in the last several years in the stimulation of new ideas worldwide. Perhaps the most striking attribute of the CTR program has been the radical increase in the pace of the new knowledge. I suspect that this rate-of-change factor

is a significant source of discomfort for many experimentalists. CTR's success in developing new ideas and increasing the pace of new knowledge is not just because the work has been primarily computational. It also has to do with the way in which the research is being conducted by the center. I believe that the atmosphere of collaboration fostered at CTR promotes open exchange of ideas and information providing remarkable stimulation of individual creativity.

While healthy competition can also stimulate individual creativity, all competition is not automatically healthy. One of the keys to the Japanese' success in the marketplace may be that they know when to compete and when to cooperate. Perhaps greater willingness to embrace collaboration in this country would improve our ability to make progress in an environment in which there are limits to budgets and therefore, limits to growth. An overall strategy for living within these limits to growth may well be necessary to continue to advance our knowledge and understanding. Stronger interactions with colleagues and shared use of resources may not only be inevitable, but, approached with a healthy attitude, may also lead to greater creativity and productivity.

Within the funding agencies, it is most important to focus first on the strategic investment of existing resources, and not to become overly consumed by budget reductions, even though they are quite significant. How to increase the level of these resources is our secondary concern. We also need to recognize the need to work together across agency lines. Program managers in fluid mechanics at the three DOD services and at NASA have been meeting regularly for quite some time now to coordinate our programs and to discuss areas that may be overlooked between us or areas that may be oversupported. These discussions help us optimize the value of the resources that we have.

One of the things to come out of these discussions is the possibility of jointly supporting research on turbulence modelling. It is our basic feeling that such a coordinated program would allow the best possible use of limited resources to address the general need to advance new concepts in turbulence modeling.

Interagency coordination is difficult, just as scientific collaboration has its difficulties. There is much to be said in favor of diversity in programs as well as diversity in research. Diversity and a lack of harmonious agreement are, in fact, signs of a healthy research community. I am not suggesting that everything be collaborative nor that our programs become directed by consensus among the agencies nor by the consensus of the research community. Coordination and collaboration do not imply consensus. They do imply communication and discussion.

Within research institutions, escalating indirect costs are eroding the competitive position of some universities - particularly at some of the top universities with their high overheads. This problem has to be worked by each and everyone of you, and is just another in the long list of strategic issues you face as a community.

Making A Difference - A Challenge to the Research Community

Having raised a number of issues, let me now offer a few things that may help (Fig. 8). First of all, there has to be a willingness to change. If you keep doing what you have been doing, you will keep getting what you have been getting. There needs to be more collaboration and cooperation. We need to build bridges between the basic and applied research communities. I urge you to take a leadership role in transitioning your research

results to the applied research community, not just to each other. We need to develop a community-wide strategy, especially for advocacy. You can start by advocating your turbulence research to the agency program managers. Many of you do this already. We continually ask for that sort of input because it helps us to advocate our research programs back in Washington. It may also be important to consider writing about what you do in popular magazines and journals.

There are perhaps things that you can do to advocate your field of research throughout government, but it is essential that this be carefully considered. A community-wide strategy may also be useful here to insure that you understand how your goals fit into the national context.

Within your own university or laboratory you have to advocate the value of what you do, and I think you have to teach the value of research in turbulence. If possible, get past the habit of restricting Phd theses to finite, workable, safe, secure problems. It is not clear that there is a very good match between that approach and the need to make the results of the research technologically relevant. Neither is it always clear that this approach really advances the frontiers of science. Let me add that all of us are busy, much too busy, and we need to reconsider the value of some meetings and some publications.

In closing, I would like to leave you with something I picked up somewhere, sorry I don't remember exactly where; it is not mine: "If you believe you can make a difference, you are right. If you believe you cannot, you are also right."

**SUPPORT AND STIMULATE BASIC RESEARCH
RELEVANT TO FUTURE AIR FORCE MISSION REQUIREMENTS**

Research must:

- be directly related to explicitly stated long-term national security needs
- be oriented toward advancing the technology base
- provide a strong scientific base of fundamental knowledge and new ideas

AFOSR shall work closely with the Air Force laboratories to transfer extramural research results to the exploratory development programs of the laboratories.

Fig. 1 The AFOSR Mission.

TURBULENCE - TECHNOLOGY GOALS

*Develop Advanced Methods of Prediction and Control
for Improved Aerodynamic Design and Enhanced Flight Vehicle Performance*

- **Mixing Control:**
 - Improved combustion
 - Chemical and Gasdynamic laser performance
 - Aero-optical performance
- **Heat Transfer Prediction and Control:**
 - High performance turbine engines
 - Aerothermal loading in hypersonic flight
 - Signature control
- **Turbulence Prediction and Control:**
 - Reduced drag, greater range and payload
 - High lift, separation control
 - Thrust vectoring

Fig. 2 Some Technology Goals for Turbulence Research

AFOSR TURBULENCE RESEARCH

***Turbulence Physics: Produce basic knowledge and understanding
Stimulate new concepts and questions
Computation, experiment, Theory, and Analysis***

- a. Transitional and organized flows:
Receptivity, stability theory, low-dimensional models
- b. High Reynolds number flows:
Asymptotics of small scales, spectral dynamics
Large scale dynamics, Turbulence production
Complex geometry, non-canonical flows
- c. Turbulent Mixing:
Entrainment control, small scale mixing, chaotic advection
- d. Other AF Relevant Flows: (Aero-optics)

Fig. 3 Turbulence Physics

AFOSR TURBULENCE RESEARCH

***Physical Process Modeling: Representation of the net effects of complexity by
models of sufficient simplicity that state-of-the-art analytical tools
can usefully predict the net response of a complex system to:***

***Parameter variations
Environmental disturbances
Control inputs***

(Requires a priori identification of the level of desired information)

Fig. 4 Turbulence Modeling

AFOSR TURBULENCE RESEARCH

Control of Turbulent and Transitional Flows: Explore the interdisciplinary frontier between fluid dynamics and control theory to develop useful methods for engineering design and application.

- a. Models: Reduced representations
- b. State estimation: Sensors - what, where, when?
- c. Controller Design: Actuators, finite dimensional approximations

Scientific Visualization and Diagnostics: Develop concepts and tools for acquiring, analyzing, and interpreting high dimensional information about complex systems (multi-parameter, multi-dimensional)

Emerging area? Nearly unsupported.
Concerned less with hardware than with concepts for analysis

Fig.5 Turbulence Control and Diagnostics Research

AFOSR FLUID MECHANICS - FUNDING TRENDS

	Core	URI	Instr	Total	Core Research Initiatives
	\$M	\$M			\$M
FY 86	9.7		0.8	10.5	1.0 Maneuverability
FY 87	9.1	1.7	0.7	11.5	
FY 88	7.7	1.5	0.7	9.9	0.5 Hypersonic Flows
FY 89	8.3	1.5		9.8	0.4 Flow Control
FY 90	8.1	1.1		9.2	1.0 Turbulence Simulation
FY 91					0.6 Heat Trans / Chaotic Advect
FY 92					0.8 Compressible Turbulence

- Five year trend, 20 % erosion of funding level.
- Increasing costs (estimate) over same period, 20 %

Fig. 6 Funding Trends for AFOSR Fluid Mechanics Research

THE CHANGING RESEARCH ENVIRONMENT

Global sociotechnical and political changes

Enhanced European collaboration
Japanese lead in Technological Innovation
National technical/industrial policy development?

Technical Environment

Wider parameter ranges (High speed, Compressible)
Complexity (geometric, multiparameter)
Contemporary research issues more complex.

Rapid, extensive changes at funding agencies?

Merger of DOD Basic Research Agencies?
Increasing emphasis on technological relevance.
Massive DOD budget reductions?

Fig. 7 The Changing Research Environment

Some Things That May Help:

- **Willingness to change**
- **Better collaboration, cooperation**
- **Bridge the basic/applied attitude and communication gap**
- **Develop a community wide strategy**
- **Advocate fluid mechanics (turbulence)**
 - With agency program managers**
 - In popular magazines and journals (other media)**
 - At all levels of government**
 - Within your own university or laboratory**
 - Teach the value of research**
- **Spend more time on analysis**
- **Reconsider the value of some meetings and publications**

Fig. 8 Suggested Action Items for the Research Community

Discussion:

Fazle Hussain: What can AFOSR do if the turbulence research community wants national research "centers"?

McMichael: It is not clear, but it certainly depends upon the magnitude. Looking at the funding, AFOSR's turbulence program is about \$3 million, the other fluid mechanics programs at AFOSR are about \$6 million, and all the other DOD fluids programs total \$10-12 million. "I cannot see taking that amount of money and putting it into centers. There has to be a balance between the need for centers and the need for individual research programs." The reality is also that the contemporary issues that turbulence research programs are addressing are sufficiently expensive that they can't be funded at very many places. "That suggests that we need to collaborate and cooperate, which may lead to some 'natural' small centers with a few people working together."

Anatol Roshko: "I'd like to make a comment on your remarks about centers--particularly CTR, which has been a resounding success and is a model center. One important thing to realize about CTR is that it grew organically--not by edict or legislation." There were a group of people at Stanford and NASA-Ames who were already involved in turbulence research and who were interested in working together. They saw an opportunity and acted on it. "To try to legislate centers, like NSF has been trying to do, would be a disaster."

Brian Cantwell: The groundwork for CTR was made in the 1970's when NASA-Ames made a commitment to the idea of simulating turbulence.

McMichael: "To abstract the CTR situation, there were a couple key ingredients present. One was that you had several people who recognized the opportunity that existed because of the Stanford/NASA- Ames relationship. It also required advocacy on the part of those people to the NASA management, and it required courage and foresight on the part of NASA management to embark on a rather radical arrangement with a university."

Dennis Bushnell: "CTR arose, like most things, because of a need-- a tremendous need to milk the turbulence simulations far more than they were being milked at that time; you needed a critical mass to do that. The other need was to get on with the business of turbulence modelling which was lying dormant at that point--and we could see that all the CFD in the world wasn't going to do much for us unless we got on with the turbulence modelling issue. It had to be done at NASA- Ames because they were doing the CFD at the time--and they were doing the simulations. It was then just a matter of creating a critical mass, and Stanford came in with an absolutely wonderful arrangement. But CTR was created because of a need--an absolute crying need. It had to be done."

Rabi Mehta: "I fail to see why a similar thing cannot be done for instrumentation or whatever we decide is important. I thought that was part of NASA's charter--to supply technology, and that was how CTR was presented to NASA management. If we can show a similar urgency, I don't see why we can't do the same thing for instrumentation."

Bushnell: "It probably can be. There are now, in effect, centers for advanced instrumentation. Hanson has one at Stanford, Miles has one at Princeton, McDaniel at Virginia, Exten (?) has one at Langley, Patalka at Sandia, and Adrian is rapidly creating a critical mass at Illinois. These things are in existence. They are informal, but they are able, because of their station and of what they produce, to secure enough funding to create a critical mass. If you want to establish centers, you would presumably have to start with those sorts of nuclei.

Cantwell: The issue of competitiveness must be addressed, and collaboration is the key in this area.

David Walker: "It seems that there is a lot of strength in diversity and competition as well. You mentioned that the Japanese know when to cooperate and when to compete. Would you elaborate on that?"

McMichael: That is probably something that Steve Kline can answer-- he has studied that quite a bit more than I have.

Bert Hesselink: Have you given any thought to other possible ways of increasing funding for turbulence (other than advocacy)? The area of instrumentation presumably is useful, and small and large companies may fund this sort of activity and create a new source of revenue. I think there may be ways in which non- traditional funding can be gained, such as SBIR's. What is your view of what AFOSR would be willing to do to help in this regard?

McMichael: We must operate within specified guidelines--to some extent we don't have a lot of flexibility. That does not mean that industry groups cannot join together and work with some organization like AFOSR. Some arrangements can be worked out--perhaps with respect to the turbulence modelling issue. I'm not sure this is something that AFOSR should foster, or if the turbulence community itself must work out.

Walter Lempert: These new diagnostic techniques, which generally cost a great deal of money, are applicable to an enormous range of problems. There should be a way to spread the cost of these techniques, not only within various programs of AFOSR, but also to multiple agencies.

McMichael: I encourage P.I.'s to look at a variety of funding strategies. One approach is to think of the instrumentation as the centerpiece of the laboratory, with different experiments set up to use the equipment and associated expertise.

Hussain: I have a concern about creating instrumentation centers. I think they are best developed and they serve their purpose best if they are an integral part of fundamental research, as opposed to being the sole focus.

Seymour Bogdonoff: Do you think facilities are available to do turbulence research as you see it in the future?

McMichael: "Sure, facilities are available. As you alluded to the future, we have had a lot of discussion that we need to do high- Reynolds-number research--we are trying to recognize that need. For example, at I.I.T. a new facility has been built. This is only one step. It is not super-high Reynolds number, but it does bridge the gap between existing laboratory research and the larger-scale government and industry lab testing facilities."

Bogdonoff: "How about speed range? We have had several people talk about compressibility and things of that sort, do you think that there are facilities in the government or elsewhere that can be used for research in that area? We have one supersonic quiet tunnel--do we need that to do supersonic turbulence research? If so, is it all going to be done at Langley?"

McMichael: "I don't have the answer to that. A lot of what is done and where it is done should be up to this community. You all have a role to play in this, but you can't play that role if you're all going to be a bunch of isolated, independent researchers, each looking out for their own particular interest. You must work together."

Cantwell: You stated that turbulence is not a 'national problem'--and I think we all agree with that. It certainly is true with regards to turbulence as it affects aerodynamics. But in the future, turbulence will be a national problem--issues of global warming and the environment are really going to bring it to the front.

McMichael: I agree with that--that is what I tried to convey: turbulence is a component in energy, the environment, and defense. It is a national problem in these regards, but I don't ever see us having a cabinet-level 'Secretary of Turbulence'.

Spiro Lekoudis
Office of Naval Research

This contribution (pp. 93 - 94) was not available at the time of publication.

Katepalli Sreenivasan (Yale University):

Sreenivasan made a short presentation which focussed on the concept of an advocacy group for turbulence. He read the text of a letter which was to be mailed to members of the turbulence community, where they were solicited to give their input regarding an advocacy group. The letter was still in draft form, and therefore it is not reproduced here. The final letter was mailed out January 4, 1991, over the signature of John Lumley. The full text is given in Appendix C.

Garry Brown (Princeton University):

I have really had a hard time thinking about something to say for this meeting. What I have to say is a matter of judgement, and matter of opinion, and the question is whether there is a level of consensus about some of these things. But, I tried to set a framework, first in my own mind and then to make judgments within that framework.

I suppose I should begin by saying that there are some really major strategic developments that can be seen as international (Figure 1). One of them is quite clearly, the post-Cold War with competition for competition for international markets. Some say that the Cold War might be replaced by a kind-of Industrial War. There's already an increased volume of international trade. Productivity is the critical determinant of the national labor rate or the exchange rate. And the development of an application technology is at the heart of productivity. Now, those kinds of views are not confined to this country, they are international and found in other countries. The other strategic issue that's quite clear is that the environment is a political issue of a much greater dimension than it was even five years ago.

Coming out of that, I think some of the things that have already been said reflect those two national issues.

I take the view that technology is the driver that we should focus on, and I think that is where we will be successful in continuing the funding. Drivers of turbulence research (Figure 2) include: civil transport, defense, energy and environment, and space.

Well, that's a very broad framework. Where do you come down to from there? If you say that they are going to be the technology- drivers then it will come down to saying something like this (Figure 3): more specific drivers for turbulence research. Their hopes, and I think their vehicles: I am thinking really of aircraft, submarines, automobiles and trucks, and military van vehicles where the issues are forces, moments, aeroelastic problems, heat transfer, noise, and control. Engines: where the issues are heat addition, heat transfer. The other feature, of course, is work extraction and work done on the gas, and perhaps the critical new element for us to, at least, appreciate and accept as a driver is emissions.

The third specific driver (again, taking a broad view) is materials processing and manufacture. I picked up a few things there that I think are probably increasingly recognized as important areas of turbulent research.

The other broad driver to which we might respond to the major trend is important and perhaps needs to be fleshed out a bit is the role of models and simulation. In the case of the F-16, and I do know that aircraft design was finalized after they completed 40,000 wind tunnel hours of testings and had gone through 135 models. The other remarkable development that is happening is that the next generation of helicopter (and I have this from the people who know) will be flown in its mission probably before metal is cut. That's to say the models, at least from the point of view of making preliminary acquisition decisions are critical to making those judgments.

I think if you look at what's happened in complex systems (and aircraft are a good example), if you look at what the situation was 20 years ago, where we were in solid mechanics, and what you could do basically based on simple ideas plane sections to remain plane, and you look at what's being done today in civil and military aircraft, the change is overwhelming (Figure 4). The role of the numerical models in solid mechanics has been tremendously significant. The point is that models and simulation allow inexpensive and accurate judgement and they do potentially provide accurate exploration of optimal design. The key thing, I think, is reduced development time and cost of development. Good models, in other words, coupled with computer-aided manufacturing are keys to superior performance and high productivity. It's not to say that that is the only thing, but they are keys. So, turbulence, in a sense, ought to be seen as a part of that key strategy to improve productivity. You don't really do it by making people work twice as hard, although there are ideas about better management and so on. The heart of the matter is making people smarter, and that really comes about, in large measure, from better models, better simulations, and better manufacturing processes.

Now, that's reflected in some national trends, and I think this is an important document - called the Defense Critical Technology's Plan of which there are 22, and those that are unclassified are these, and there are 20 of them (Figure 5). And there is a plan for these critical defense technologies. These are the technologies which are seen by a wide community. They are not dreamed up by individuals, they have had quite a high level of endorsement by a wide group of people. The ones that are of interest to us in particular, are parallel computer architectures, computational fluid dynamics and air-breathing propulsion. The other thing that's interesting is how much money is going to be spent on those things in 1991. I think that strategy reflects what I said previously about the significance of prediction and models.

Another quite interesting national direction that we set in 1983 is shown here (Figure 5) but it is true and really is derived from those strategic drivers that I mentioned previously, or they are derived from it. In a major review of aeronautics, George A. Keyworth II, the Scientific Advisor to the President made this statement: "We simply can't allow ourselves nor will we accept any option in aeronautics other than pre-eminence. Its immense strategic potential, particularly for defense, dictates a vigorous research and technology plan and demands a continued and strong Government involvement to ensure that adequate national investments are made in research and technology." I think one would broaden that to say its strategic significance is not only for defense but perhaps particularly for industry since most recognize that U.S. enjoys a comparative advantage and must sustain a comparative advantage, economically.

Another statement that I think is also of real significance for us is this statement by President Bush: "Let us remember as we chase our dreams into the stars that our first responsibility is to our Earth, to our children, to ourselves. Yes, let us dream, let us pursue those dreams, but let us also preserve the fragile world we inhabit." I do believe that the environment is a major driver for turbulence research, and this is Bush's statement recently. I think there is increasing public will to do something about the environment.

Another national direction that I think is interesting and consistent with that (Figure 6) is the Aerospace Industry's Association establishment in 1989 of The National Center for Advanced Technologies (NCAT). That is a foundation for integrating and coordinating key technologies for the 1990's programs and assisting in its implementation. And, in a way, what we are talking about is a subset of that. They have listed key technologies with a view to

developing strategic plans for those technologies. They overlap, in fact, this is a sub- set of the critical technologies for defense. Fifteen of the twenty critical technologies for the defense I put out map into these. Again the emphasis is on air-breathing propulsion and on computational science.

I think the other thing to appreciate is that on one of the reasons why funding has been cut in some areas because there are a lot of other competing technologies. A lot of progress is being made in these other areas with very high payoffs. To some extent, it is surprising that we have done as well as we have.

The principal objectives (Figure 7), - it is worth noting what they are, because again I would say we ought to be thinking of how we couple in with this larger community. One of their drivers is aerospace. It's to develop national consensus and support for Key Technologies; it's to support adequate and stable funding in the federal budget for an adequate technology base, and also for specific Key Technologies; to utilize industry and government to adopt the Key Technologies development plans as their strategic research and development plans; and to provide counsel to the government agencies and to others, and to act as an impartial bridge between industry, the administration, and Congress.

Again, I would say if we wanted to be effective and couple into national mechanisms, I believe that this is one, just as the defense is one, in which we ought to place our hat, and be involved, and have a voice.

I then turned to the real subject of the talk that I was supposed to talk about, but I did so in the context of that frequent strategy. Strategies for effective turbulence research in a changing environment - the first question I thought was an important one is Big Science versus diversity (Figure 8). We have heard the advantages of Big Science - they are certainly critical mass, high-profile, major equipment investments, and big goals that are identified. The advantages of diversity are that it is responsive, it is intrinsically smaller scale, less bureaucratic, responsive to good ideas. It reduces unhealthy prejudice. Big organizations are frequently dominated by individuals who can't handle unhealthy prejudice. It tends to support good people and groups with a good track record. That is very good. Research is coupled with teaching. In my mind that is very important, and I speak with some experience there, having been outside of a university environment for the last two years. And there are many targets and frequently when you're not exactly sure where you are going, a little diversity in targets is very healthy.

My conclusion, frankly, is that Big Science is easier to start than it is to turn off. These things have a decay time, they do tend to get heavy, do tend to get bureaucratic, the initial excitement somewhat wanes, and the people may not be as good as the big people who started and then went off to do other exciting things. I have considerable nervousness about the idea of an institution or national center which involves a great big infrastructure. But what I do say is that I favor the diversity, but I do think we can try some initiatives.

Before you can talk about some initiatives, you really want to ask, I think, what's the key problem and what's the key payoff? What do we really want to deliver? Any why can we deliver it better if we have more money? I believe the key challenges are: better physical understanding (large order effects), and better predictive capability through CFD. They are essentially to provide better physical understanding. There is no doubt in my mind that that is a deliverable that is different from better predictive capability. It is another aspect of predictive capability. But better physical understanding has already had a high payoff. I do believe that it is a deliverable of itself and it is a goal of itself.

Better predictive capability through CFD is, quite clearly from all that I have said before, ultimately the heart of the matter. We ought to be directing this enterprise over the long term at really developing numerical capability. Now, to do what? I really haven't said that. I have said that within those two things - better physical understanding. In what areas? And why?

I went back to those strategic drivers, and I said if we really are to make an impact on those strategic drivers, (going back to vehicles, engines, and materials processing). What are the important areas to gain a better physical understanding (Figure 9)? And how would I derive them? I don't want to draw a one-to-one comparison, but I can honestly say that I sat back and looked at those things and said that's what we'd like to do better in the country. My view is that these are key areas which contribute to being able to make a contribution to that.

High Reynolds number mixing and combustion, and that is in the context of transport, energy and environment, heat transfer, compressibility, unsteady boundary conditions (in most of the problems where you are trying to extract work or do work on a fluid you're talking about an intrinsically unsteady problem). Three-dimensional separation is obviously a critical aspect of vehicles and I'm thinking specifically of aircraft and submarines. It's defense-driven, perhaps more than some of the other areas, but it does have a major pay-off in the defense projects. And more generally, I do believe that we need to understand flow fields where mean vorticity is neither parallel nor perpendicular to the velocity. Most problems are complex problems where those things don't apply, and yet most of our intuition and our analytical capabilities, and to some extent, our experiments, have been coupled into problems of this kind. Now, why do I believe that there is scope for major advances in physical understanding? And I do believe that. I think today we are poised for major advances and we could put our hands on our hearts and say we had some past successes. It is a great surprise to me, in a way, but it is also illuminating that those riblets still work at a Reynolds number of 3×10^8 . And the only reason anyone ever dreamed of the idea of developing these riblets was because they looked at the structure and said there's obviously a longitudinal streaky structure in this flow - can you do anything about it? And the answer is, if you do this crazy thing, put ribs on the surface, (not withstanding how small they are), at a Reynolds 3×10^8 , you make a 4% difference to the drag. That pays for our enterprise in terms of national payoff. This is enormous. If that gets supplied to all 747's around the world, as it may do, there would be a tremendous payoff.

I could talk of mixing layers. I think there have been enormous advances in conceptual understanding that have come about through an appreciation of the intrinsic physics of mixing layers.

But, the other reason for optimism and the belief that we can deliver more is because I think there has been an order-of-magnitude change in instrumentation capabilities through optics and computers. They are a different order form 10 years ago. I think that CFD, as a contribution to physical insight, is an order-of-magnitude improvement over the sort of tools we had available to us one or two decades ago.

Strategies for both predictive capabilities through CFD (Figure 10). I have listed what I consider to be four key things. I do believe we must provide a direct overlap between Direct Navier-Stokes simulation at low Reynolds number and benchmark experiments. There remain questions in some people's minds about whether those solutions are truly solutions, and I think there ought to be an overlap there with the experiment. Then we can go from there

with the experiment at a Reynolds number and watch and maybe, in fact, adulterate to some extent those calculations and see how well we can really advance to higher Reynolds number. I mentioned the other day in a meeting, I think it's an interesting possibility where you halve the mesh size and demonstrate there's no change in dissipation. At that point I think you could be fairly satisfied that you have the solution. Now, halve the viscosity and you don't have to compute for a long time to look again at the balance between large scales and small scales. So it's not necessary to compute for the long times that you used to get a lot of insight into the Reynolds number. So, I think there are other ideas there. I think quite clearly that Direct Navier-Stokes simulations can not go to higher Reynolds number and meet the real technological problems. There has to be interaction between theory, current numerical results and future ones, and higher Reynolds number experiments, essentially directing ideas for what has to be a large-eddy simulation of some kind. What seems to be quite clear is that Reynolds-averaging fails essentially because it does not deal with the large-scale, low-frequency motions which feel the boundary conditions. And we will not really solve free shear problems until we have models that deal with that issue. That is the stress-producing part of the motion.

We have to develop, I believe, strategies for the inclusion of chemical reactions. Again, we go back to those national drivers in the context of the environment and develop some kind of strategy for that. I think the current models have been more successful than many would have believed. I think perhaps a realization that they have done that has enabled CFD to advance significantly further than one might have expected.

There are new ideas and I think that there is scope for further development for those models which may provide a firm foundation and a wider range of applicability. I do believe that the current inadequacies of those models need to be better quantified. How well do they do in three-dimensional flows and what really fails?

I think that there is, again, quite obviously scope for major advances because of the increase in computer power and the case that one can make if you have to argue historically in terms of rate of progress. It has been very high and increasing and I think that is why that is a telling argument.

So, in summary (Figure 11), I have some ideas for initiatives. As I said, I don't really myself, and that's a matter of judgement, support the idea of a national center which involves a big infrastructure. But at least, I would need to be persuaded before going that far that we have exploited other possibilities to get into high Reynolds number. I think there are some very-high-Reynolds-number facilities in NASA. It has to be established whether or not they could be used and whether they could be used in imaginative ways and with various collaborative programs. They are very expensive; but if you are talking about a national center, I think you are talking about \$10 million a year (not including, perhaps, the original initiative investment) and you can do a lot in those facilities with \$10 million a year. So, I think that is the avenue that needs to be exploited. I think the CFD results really do have to be very widely accessible and we will have to make sure they are. I'm not saying anything are new, but that is important.

I think, in terms of the other areas, we really do need to evaluate, in terms of advanced numerical predictions, large-eddy simulation ideas. Are there some new ideas? Ultimately, this field progresses through good ideas. What is required is some evaluation of whether we have any. Whether there are some new ideas. Whether there are some new techniques that are very promising that through communication we might actually make more rapid progress than we would otherwise. Also believe we need to evaluate ideas for the inclusion of chemistry.

Again, coming out of the drivers that I describe, this is very important.

There are further initiatives that I might have mentioned about advocacy, one of which is to couple in more effectively in the national center for advanced technologies.

Strategic Issues

Post Cold War

- * Competition for international markets
- * Internationalization of industry
- * Increased volume of international trade
- * Productivity is the critical determinant of the national labor rate (exchange rate)
- * The development and application of technology is at the heart of productivity

Environment

Drivers for Turbulence Research

- * Civil transport
- * Defense
- * Energy and Environment
- * Space

More Specific Drivers for Turbulence Research

* Vehicles

- Forces, moments, aeroelastic problems, heat-transfer, Noise, Control

* Engines (Gas Path)

- Heat addition and heat transfer
- Work extraction and work done on the gas
- Emissions (combustion)

* Materials Processing and Manufacture

- Energy addition and extraction
- Mixing
- Reactions
- Emissions
- Forces

Role of Models and Simulation

- * F-16 - 40,000 wind-tunnel hours, 135 models
- * Next generation helicopter flown in its mission before metal is cut
- * Solid Mechanics - c.f. "plane sections remain plane" and finite element methods
- * Inexpensive and "accurate" exploration of optimal design
- * Reduced development time and cost
- * Good models coupled with computer-aided manufacturing are keys to superior performance and high productivity (but it is hard to beat a better idea!)

Some National Directions

Defense Critical Technologies

- * Semiconductor Materials and Microelectronic Circuits
- * Software Producibility
- \$120M * Parallel Computer Architectures
- * Machine Intelligence and Robotics
- * Simulation and Modelling
- * Photonics
- * Sensitive Radars
- * Passive Sensors
- * Signal Processing
- * Signature Control
- * Weapon System Environment
- * Data Fusion
- \$80M * Computational Fluid Dynamics
- \$180M * Air-Breathing Propulsion
- * Pulsed Power
- * Hypervelocity Projectiles
- * High Energy Density Materials
- * Composite Materials
- * Superconductivity
- * Biotechnology Materials and Processes

Aerospace Industries Association of America established in 1989 the National Center for Advanced Technologies (NCAT), a foundation for integrating and coordinating AIA's Key Technologies for the 1990's program and assisting in its implementation.

AIA Key Technologies are:

- * Advanced Composites
- * Advance Sensors
- * Air-Breathing Propulsion
- * Rocket Propulsion
- * Artificial Intelligence
- * Computational Science
- * Optical Information Processing
- * Software Development
- * Superconductivity
- * Ultra-reliable electronic systems

The principal objectives of the NCAT are to:

- * Develop national consensus and support for Key Technologies
- * Support adequate and stable funding in the federal budget for an adequate technology base and also for specific Key Technologies
- * Utilize industry and government to adopt the Key Technologies development plans as their strategic research and development plans
- * Provide counsel to government departments, agencies and others, regarding technology integration, planning and policy
- * Act as an impartial bridge between industry, the administration and Congress to encourage adequate and continuous support of all technology-related resources, such as manufacturing processes, testing and evaluation, and the education of science and technical personnel.

"Strategies for effective turbulence research in a changing environment"

Big Science vs. Diversity

* Big Science (advantages)

- critical mass
- high profile
- major equipment investments
- big goals identified

* Diversity (advantages)

- responsive to good ideas
- reduces "unhealthy" prejudice
- tends to support individuals and groups with a track record
- research coupled with teaching
- many targets

Conclusion: favor present diversity but some further initiatives could be taken

Strategies for better physical understanding

Important areas requiring better physical understanding which follow from technology drivers are:

- * high Re
- * mixing and combustion
- * heat transfer
- * compressibility
- * unsteady B.C's
- * 3-D separation
- * Flow fields with mean vorticity neither parallel nor perpendicular to the velocity

Scope for major advances because of

- * past successes (e.g. boundary layer and mixing layer)
- * instrumentation (optical, computers)
- * CFD

Strategies for better predictive capability through CFD

- * provide a direct overlap between DNS (low Re) and "bench mark experiments"
- * develop large eddy simulation
 - Theory, DNS and high Re experiments directed at ideas for implementing LES
- * develop strategies for the inclusion of chemical reactions
- * boundary layers
 - further development of steady models may provide a firmer foundation and a wider range of applicability of these models though current inadequacies need to be better quantified

Scope for major advances because of

- * increasing computing power
- * rate of progress increasing

Initiatives

- * Exploit high Re facilities of NASA
- * Make CFD results more readily accessible
- * Evaluate LES ideas
- * Evaluate ideas for inclusion of chemistry

Dennis Bushnell (NASA Langley Research Center):

I would just like to say a few remarks from more of a mission agency. This will be very brief. The hour is late.

Forecasting the Federal Budget is much like first-order weather forecasting: tomorrow will be just like today. There are two issues at this meeting. One is what to do in the future with the current funding levels, and that's what we talked about mostly yesterday. The other issue is how can we get significant funding dealt us? And I would like to address this second issue.

The current funding situation - turns out Lumley was right after all; the amount going to turbulence is approximately the same even in the face of the need to fund these new exciting things. The amount, however, going to individual researchers for fundamental research has come down. It has come down for many reasons, unfortunately. As far as NASA is concerned, a lot more of our money over the past 10 or 15 years has gone into in-house manpower (on-site contractors) because we were not able to hire civil servants for the skill mixes that we need. We had to use R & D bucks for those, and every one of those people is 80-100 K, and we have in many cases 30-40 % of our research staff of these people. Ordinarily, a good piece of that money would have gone to universities.

CTR is a wonderful thing. It's also an in-house, on-site effort at Ames which is funded out of the same money that would ordinarily go to individual researchers. AFOSR, ONR, ARO have U.R.I.'s that are wonderful. They were established at the request of the university community to get more money into the centers. Congress said do it, without providing any more money. So a lot of the money for those was subtracted out of the common fund which would have gone for individual research. NSF has been driven by Congress to support technology-driven science and you understand what happened there. I have no personal data on DOE and E.P.A. Funding levels required are increasing due to the increased sophistication of instrumentation, facilities and so on. So there is a problem, absolutely.

To increase the funding levels for individual investigators, I think you have to do one of two things - at least in my estimation.

1. Is to find and support a new application which is critically dependent on turbulence. We've done that recently for the last couple years at Langley, as far as NASP. We have used it to put in a 2-3 million delta for our turbulence research and that will stand for a couple of years until we hop onto the next bus going by, which will be the SST.

2. You can also identify problems and establish a research program which will provide major benefits to the U.S. Some suggested things here that would catch people's attention - you should probably double your budget overnight if you took on these types of problems. Perhaps even triple it (this is what Spiro Lekoudis was talking about). If you want, work at controlling the stability of fusion reactions so you give the people fusion. Reduce turbulent drag on the order of 50% - 50% of the world's turbulent skin friction is from this number,

in terms of economics. Reduce diffuser length by a factor of 3. Right now, fighter aircraft are just sheet metal wrapped around supersonic and subsonic diffusers, and they cost so much a pop. Predict microbursts and tornados, if you want to really save money for the U.S. Reduce pollution and improve internal combustion engines by the order of 30%.

Now, what have we done? Previous to this, what can we crow about? There are a few things. In the late fifties and sixties people wanted ICBM's and manned spacecraft and it turns out they were burning up. The Germans found this out - the V-2 issue. We had to measure turbulent Stanton numbers to figure out how to design these things for survivability for aero heating. That's a very bright success story for experimental turbulence research. We figured out ways to control Karman shedding on oil-rig platforms, chimneys and all kinds of other things. We have built improved diffusers. We have taken some data which has allowed numerical weather forecasting. Polymer drag reduction has been very, very useful. The calibration data for turbulence modelling has helped us as far as design. We need to do more of that. Very efficient high-lift systems. All of you could probably come up with other additions to this. Experimental turbulence research has been very useful.

A lot of these things are gadgets. Some of these are new buses going by, and if you really want to put in a major increment, Spiro, or I, or any of us in the government know where to go, know which levers to pull, know where the cash drawer is, and what we need is some ideas on important problems and we can pull the money out for you. It's that simple. It works that way.

Richard Wlezien (IIT):

I know it's late in the day; I know after 2 days, the last thing you want to hear about is another center. The difference here is, first of all, the center is based on a facility and there really hasn't been a lot of talk on facilities today. And the second thing is, the facility is real. This is not a proposal. The original proposal for this facility went in 6 yrs ago, and in 6 months this facility will be operational.

The motivation was high Reynolds number. A few features: one is that it be a high-quality facility (low-turbulence level, low acoustic level), and it is designed for the study of high-Reynolds-number flows by going to something large. So it's a much larger facility for fundamental research than you normally see in the university. It's based on the premise that all large facilities - and I hesitate to say all, because someone will probably correct me on this - but most large facilities except the NTF were designed in the 1940's. They were not designed for low-turbulence levels. And so they generally have poor flow quality and are not designed for doing fundamental research. They are expensive to operate, and as Gary Settles mentioned yesterday, the unions beat him to death when he tries to run these facilities. I would contend that it's not necessarily a feature of large facilities, but it's the fact these facilities were designed for testing, not for research. I have tried to do research in testing facilities and it doesn't work. This was designed as a research facility, and I will tell you some of the features in a little while. These areas are some of the focal areas for research that will be done by people from IIT, but the facility is by no way limited to just those features. The idea is to go to 3-D, high Reynolds number, turbulent boundary layers, (there will be a turbulent boundary layer thickness in this facility on the order of five inches or more), multiple body problems, far-wake, and so on. The main feature is high Reynolds number.

Now, the fundamental background for this is the fact that if you look at the range of Reynolds number and Mach number, you have the applications somewhere up here and the large, noisy, dirty tunnels at the various government agencies fall somewhere in this range. And then there are the tunnels that we can all afford, which are somewhere down here. There is a gap in the middle. So the idea was to design a facility that would essentially fill that gap. That is the range of Reynolds number and Mach number that this facility is designed for, and so, as you can see, there is a unique quality to it.

As I said, the tunnel is virtually completed. This is what it looks like, if any of you stop IIT, you can see the facility. Again, this is really Hassan's (Nagib) child, in a sense. He pushed for it quite a long time ago, before people were really talking about national facilities.

The tunnel has a test section that is 35 feet long, the cross section is 4' high by 5' wide with a removable top so that you can put in any sort of pressure gradient that you would need. The facility is completed essentially from this diffuser through this turn and also the contraction is in-place. What needs to be completed yet are the screens and the flow treatment as well as the test section. The facility has been completely designed. The start

date for its first running will be next March. But, in fact, we are already blowing air in the facility. This has probably been the most thoroughly diagnosed facility of its kind in that it's not a facility that's going to be fixed after it's built, but it's being tested all throughout the construction process. Detailed data has been obtained all throughout the loop. The fan system has been diagnosed, the corners have been diagnosed, etc.

The thing I want to emphasize is not that it is not an IIT facility, it's a national facility that happens to be at IIT. As far as we are concerned, this facility belongs to the turbulence research community, and so it is up to you to decide how you would like to use it. In fact, we'd like to solicit some opinions on how decisions need to be made on the use of such a facility. Again, I hesitate to use the words "National Center", which has been knocked around so much today. This slide is out of the original proposal from 6 yrs ago - so this is not a new idea. It will be geared to be used by people from other universities and by industry and it will be a cooperative effort. This will not be an individual sort of thing, people will come in groups to use it. It's being designed so we can get experiments in and out of the facility quickly. There will be a support staff to support you while you are there, and it will also be designed to be a low-cost facility. As you can see, these are the features. As I mentioned: low- turbulence level, you can specify pressure gradient, totally computer controlled. So essentially you will be able to interactively dial in a pressure gradient and the upper wall will adjust itself to give you the pressure gradient you need for a particular experiment. Chicago is major metropolitan area and so it is easy to get there. The group is established; we are now seven researchers, so again it is not a one-person operation.

Also, the data acquisition and processing is designed to give you fast turn-around with graphical output, so it won't be a test. In testing facilities, you prescribe everything in advance, you let some technician run it for you, and a month later you come back and find out all the data is bad. Here, you will be able to interact with your experiment on-line, make decisions on-line and really do research as opposed to testing.

Those were the original specifications. The current specifications for the tunnel look something like this. Maximum speed of 500 feet-per-second; it is actually a 2,000 HP electric motor that drives the system. That's all in place, 4' x 5' test section, as I said. A test section insert is being designed to get into the transonic range - or low transonic, 750 feet per second. The projected turbulence intensity (now this is based on surveys before the final turn and the screens are installed) is in the range of .03% - .05%. So it's really a very, very clean facility. It's as clean as just about any other facility that you can get into. It has been designed to be as efficient as possible. For example, heat exchanges are integrated into the turning vanes, and there is an ice bath system. I won't go into the details of it, but it will give you a controlled temperature over the duration of the run. There is a chilled water system, 1100 ton- hours of ice storage. This is just a decision that was made based on efficiency, so that we wouldn't have to go to a big air conditioning system. It will run steady-state for test section velocities of up to 300 ft. per second. The other option is you freeze ice all night, come in in the morning, and then you can run for 2 hrs at the top speed of the tunnel. So it depends on what sort of operation that you want to run. It's also designed for low acoustic levels in the test section. The fan was specifically chosen to be low RPM with variable pitch. Again, the pitch of the fan will change under computer control as you change test section velocity to keep that noise level low. Sound absorption materials are integrated into the last turn instead of the turning vanes. We're looking at some other options to make the acoustic level as low as possible. I saw it run for the first time a few months ago, and even without a test section I was very impressed with how quiet the facility actually was compared to other tunnels.

Quickly, these are some of the other features. A very specialized test section - the test section walls are actually being designed so the maximum deflection is on the order of a 32nd of an inch, and the test section itself was very heavy construction so there won't be these vibration issues that you run into in a lot of other tunnels. The traversing system will be integrated as I mentioned, and the movable ceiling panels.

One other feature is the fact that the test section will be segmented in 8 foot sections, and this first section will be removable. There will be at two of these constructed, so one test section can be out and available for installation of the model while the test is in progress.

The other feature is that the traction can be removed so that routine maintenance can be done on the screens. Screens are something you talk about taking care of and no one ever really bothers with. The idea is to get in there at least once a year and clean the screens so that for transition-type measurements you know that you have a good clean facility. So, these are all issues that have been addressed up-front in the design of the tunnel and are not being handled after the fact and won't be a problem in the operation of the facility.

This is the key slide that I want to show you because this is the plan for using the tunnel. Again, we are open to all sorts of suggestions on this. This is just a preliminary plan, the idea is that the group at I.I.T. will use this tunnel no more than 1/3 of the time - that the rest of the time would be open for the fluid mechanics community to have access.

Now, as long as we have our U.R.I. support, people whose work is approved and is consistent with the goals of the Center will have free use of the facilities. That is you won't pay for electricity, you won't pay for technicians. You are going to pay for is your fees while you are in Chicago and also the cost of your model. So, it is a very cheap facility in that sense, and even if you are doing something, say with private industry which is really not of fundamental interest to the research community, then it's a relatively nominal fee of \$1,000 a day to run the facility. That is the cost of electricity, technician time, and so forth. So, you don't have to worry about providing all this when you come to IIT. The other idea is that we don't expect people to have to be there for more than a month at a time. You can ship your models in, the technicians will install it in the extra test section, it will be up and ready to go when you arrive on site. You run your test, get your data, and leave. The technicians will take the model back out for you and ship it back. We talked about the issue of having people on-site and not on-site. If you are doing CFD, everything that you are doing comes back to you on your terminal. But, when you are running an experiment there's no way of knowing that a technician put your model in backwards unless you are sitting beside the tunnel.

There will be a committee established, an advisory committee, with people from the funding agencies, academia, and industry to decide what problems are to be done in this facility. At the same time you send a proposal to the funding agency, you also submit a plan to the committee for approval so that presumably when your funding comes in you will also have a slot approved. And the final thing is that technicians are non-union, so you don't have to pay union dues.

That's really what I wanted to say. I just wanted to give you some of the details about how our tunnel is constructed.

SESSION 6

Summaries and Reports From Working Groups

Session Chairman: Steve Kline, Stanford University

Session Recorder: Jim Brasseur, Pennsylvania State University

Summary by Dennis Bushnell (NASA Langley Research Center):

My assignment to prepare this, I think, was an unsuccessful attempt to keep me quiet during the meeting. It didn't work. I have three very simple charts. One chart says 'problems,' one says 'solutions,' and the other says 'action items.' I have drawn this from the proceedings of the workshop as I wrote them down, in a very simple way and that is to take a vote. If something was mentioned or appeared to be a consensus, then it is on these charts.

The problems These are recurrent themes and comments. The problems are (i) a decline in funding for individual researchers (Lumley is right), (ii) advocacy is segmented over multitudinous applications, and (iii) increased funding is needed due to increased sophistication of instrumentation and problems. These come largely out of the marching orders from Lex and they were repeatedly brought up, and so forth. This is the problem statement, and if you're going to have some solutions you need to understand what the problem is, and I think that's the problem.

The solutions are next. These are the solutions that we heard about. The first one is to work together, collaborate, cooperate^@on experiment, theory and numerics (all three together), on applications and basics, between government, university and industry, off shore and on shore. The second solution is enhanced use of computers, and this is equally universal. Use computers to automate experiments, to have interactive control of both experiments and turbulence, to do data storage, display, analysis, retrieval, transmission and anything else you would like to be able to do with the data, and also to compare with numerical solutions. The third solution is to perform large scale and high Reynolds number experiments at national centers. The IIT tunnel we heard about, and the NASA tunnels are, or can be made, available for negotiated use. There are a large number of facilities which are available and could be used with tremendous Reynolds number ranges. There was talk at one time about putting a flat plate in the NTF at, I think, 800+ million Reynolds number; see Bill Saric for the plan for that experiment. Also, combined advance instrumentation development with active turbulence research is, by in large, being done. Those are the solutions.

My last chart is the action items and these too come directly from the proceedings of the meeting as recurrent themes and comments: (i) Advocacy everywhere, always, constantly, full

court press. You can all do it, write your congressman, talk to your spouse, do anything else that you think will work; (ii) Tying in with important applications. One of the most important, easily understood applications within the technical community is turbulence modeling. There are many, many others which have been mentioned throughout the meeting; (iii) Full three space and time, four dimensional instrumentation development and in turn the interpretation techniques for four dimensional data bases be they generated by simulations or by these emerging instruments. This interpretation can involve visualization, holography, stereo-pairs and a helmet, or laser projections. The people in Hollywood know how to confer information. They cheat, they study the physiology of the human mind and senses, visualization and so forth, and we need to get on to some of these other field, perhaps; (iv) We need increased availability of the numerical simulation data bases. I happened to have served on the advisory panel for CTR, and this has been a continuing recommendation to CTR every year when the advisory committee meets. The reaction from CTR is that it takes up too much time of their good people. What that means is that we need to find a good way to do it which is user friendly. We need to run experiments at high Reynolds number, we need to run experiments with complex boundary conditions and complex geometry, and we need to tie in some of the high Reynolds number experiments with LES. Also, perhaps there is a need for enhanced creativity and some courage for taking on some very very different approaches in turbulence, experimentally.

Summary by David Dolling (University of Texas at Austin):

I'll preface my remarks by saying that around eight o'clock last night it suddenly occurred to me that I have got to learn to say no. It is very difficult to do this job, and I am going to be brief and probably offer no new insights. I think that if this meeting had been entirely successful, then we would not need a summary. I will qualify my summary by saying a couple of things: one is that I have heard this remark[1] consistently over the last decade, so if I have missed any vital points, I apologize. Also, only in the last two or three years have I been really interested in turbulence, per se, in the sense of measuring turbulence. My interest has been largely in shock wave-boundary layer interactions and unsteady separation, all of which involve turbulent flows, but I was not very interested in the intimate details of turbulence. I have also been a user of tools more than a developer, so my perspective is a little different. A third point is that I sympathize with industry, having spent some time in the British Aerospace industry, so I have a slightly different perspective.

The purpose of the meeting (and maybe we should rehash this) was, to use words from Jim McMichael yesterday, that "a slow but not precipitous decline in funding" is occurring as our ability to measure and compute things is improving. The question addressed by the workshop was "can anything evolve to prevent and hopefully reverse this decline in funding?" This, I think, is the objective that we have all met to discuss and try to do something about. Well, what have we seen in the last two days? There have been five major sessions[2]. I kept fairly detailed notes, and there were fifteen main speakers, nine other presentations and 162 "remarks" from the floor, encompassing a huge diversity of topics ranging from politics to the details of particular measuring techniques.

Now, what comes out of all this, and I am not going to get into details since it is all very general stuff, is that, like it or lump it, fluid mechanics is not a national issue[3]. (You will notice that I have tried to make my whole presentation up with quotes from the floor which I have packaged together with prepositions.) Whether we like to work in a field that is "unimportant," well that is a problem each of us must deal with. There is a big case to be made, and I think that we have all agreed that the case is not being made right now. The question, then, is how to make that case and generate excitement both within our community and outside of it. My feeling from listening for two days is that there is a consensus on the need for advocacy, but there is really no consensus whatsoever as to the mechanics of doing it. I think this is an issue we are going to have to spend a lot of time looking on. The question of what the advocate should be advocating is a rather critical one because, to me, just listening has exposed some of the weaknesses of this community. The weaknesses, I think, are summed up with the statement, "well, we are getting a better physical understanding and a better predictive capability (through CFD) but to do what?" It is not really clear to many of us what the goals are; and if it is not clear to us, it surely is not clear to the general public.

Another quote that I thought was important is that "scientific curiosity is insufficient" to increase funding or even to keep it constant. Another set of quotes which have come from a broad variety of people and I think these are again issues that, like it or lump it, we have

to deal with are: (i) "more funding needs a demonstration of the impacts"; (ii) "what are the problems, the key problems, the key payoffs"; (iii) "find a new problem critically dependent on turbulence"; I think we all recognize the one in the middle: (iv) "new ideas will open the cash drawer" (what are those new ideas?); and all of it comes down to: (v) "close the basic/applied gap." I think it was Val Kibbens from McDonnell Douglas who provided this rather nice statement: (vi) "applied problems can be a navigational aid and an inspiration." I think that is quite true.

So, the initial task of this advocacy group (and this is a circular iteration because, even though we do not know who the advocates are, I am stating their initial task!) is really to identify what the needs are. That needs to be done in conjunction with a wide constituency (ASME, AIAA, ASChE, etc.). There are many people out there studying turbulence who we probably know nothing about. When I went to Texas in 1983, I met a lot of people in the chemical processing industry and the oil industry who all study turbulent flows and all sorts of aspects of fluid mechanics that I was totally unfamiliar with. It seems evident, too, from what we are told from funding agencies, that there is a certain strength and necessity for interdisciplinary problems. One of the difficulties I see in this question of identifying problems is their diversity^@there is such a diversity of problems, not a single problem. If we use the physics community as an example, in the lay press all we ever see as the reason for the super-conductor super-collider is the need to answer to the basic question: "is the model used right?" "Is there a particle missing?" There are many other problems, but at least the high energy physics community has a single focus. I am not sure that we have a single focus.

Furthermore, the problems must be large in scope and, I think, with broad applications. Beware of and prepare for the "Thatcher response." Mrs. Thatcher's response to a lot of technology development has been "well, if it is so damned important, why doesn't industry pay for it? Why should the government pay for it?" That is something I think we need to be prepared for. We must answer why this should be done at the taxpayers expense.

Based on conversations I have heard from people in the hallway, these problems, whatever they are, need to be split into categories. There are problems with short term goals, middle term goals, and long term goals. You cannot interest people in things that are going to take ten years to get to the first solution. As an example of the type of thing that might be a short term goal[4] is the vague statement "control, or improvement, without understanding why." This type of thing is done in industry all the time, solving problems either empirically or semi-empirically, developing something that works and never completely knowing why. And nobody really cares, at the time. Medium goals and long term goals: probably the more fundamental topics that many of us are interested in fall into this latter category. My own view is that a focus on specific problems with practical applications in mind might actually fire up interest in our field amongst younger people. Nearly all of us teach in engineering schools; we are not in math departments or in physics departments. My experience is that most of the young people come into the field because they are really interested in doing practical things, and frequently our research programs choke that interest.

The second point that I would like to make is that there is a lot of discussion about facilities and instrumentation; in some sense, that is putting the cart before the horse. I think that to identify the problems is a prerequisite to the solutions (for any discussion of needs). We have heard a lot, for example, about a large high Reynolds number facility costing substantial sums of money, but is that where the critical problems really are? What about compressible flows? It seems to me that in aeronautics, at least, a large fraction of the critical problems are in compressible flows. Do we need facilities for compressible flows? These drivers, I think, are a decisive factor in any discussion of instrumentation needs and

their development. Instrumentation is something that interests me because an ever increasing fraction of our costs and time is going into instrumentation, and that is very detrimental to the training of students in fluid mechanics. (I now have students who know more about the software and hardware of our A/D converters than the companies that built them! We no longer call the company for advice because they can't offer it to us. And it is taking these students months and months to learn this, while at the same time they are getting progressively weaker in fluid mechanics.) So, I think, these are also interesting questions to address. I came across this issue a couple of years ago when somebody told me that their experiments were now able to simulate CFD. Is that really the way to go in all experiments (i.e., 10 [to the] 12[th] data points), or are there just certain experiments where the need exists. Well, you can read the last two points for yourself; they are important, but probably not as critical[1], at least in the advocacy argument.

So in summary, and this is very simplistic, I think there is a long and arduous, but potentially rewarding road ahead. There is a need for advocacy to identify problems, and for success these problems must be integrated into national programs. There has to be some consensus. That is going to be difficult. There has to be support in the turbulence community and from the general public. Only at that stage is there some framework for the discussion of facilities, instrumentation, approaches, and so forth. Until we have carried out the first steps, my feeling is that the last step is not doable. Thank you very much.

FOOTNOTES TO DOLLING:

[1] "you are not a good listener," [Susan Dolling, private communication, 1980-1990]

[2] (1) Directions for experimental research; (2) Computers and experiments; data sets; (3) Instrumentation and facilities; (4) Educational issues; (5) Strategies in a changing environment.

[3] "Turbulence is pervasive but by itself does not have a national profile, except as a component of other issues."

[4] "short term goals (clearly identifiable, understandable, and achievable) necessary to sustain interest and validate approach"

Discussion of Summaries

The discussion centered on two global issues: (1) the use of NASA facilities for experimental turbulence research, specifically at high Reynolds numbers and (2) the availability and use of the numerically generated databases at the Center for Turbulence Research (CTR, NASA-Ames Research Center).

The use of NASA facilities was discussed through a series of questions concerning (a) to what extent NASA facilities meet the needs of the turbulence community, (b) the availability of the facilities, and (c) how to gain access to the facilities. A question was raised by Brian Cantwell about the cleanliness of the tunnels, turbulence levels, and so forth. It was pointed out by Bushnell that a large variety of high quality, low turbulence level tunnels exist within NASA. However, it should be kept in mind that for some classes of experiments, low turbulence levels are not necessary. Several specific facilities were mentioned, including a low turbulence high pressure tunnel at Langley, the Ames 12 foot tunnel, a half-mile long tow tank, a 30 foot long axial flow cylinder for the 7' by 10' or other large tunnels, and a number of low speed, low turbulence level tunnels. It was further pointed out that, whereas the IIT facility will meet some of the needs for high Reynolds number turbulence research, other facilities will be desirable. Data summarizing the capabilities of the NASA tunnels is given in NASA RP-1132. A lively discussion concerning the availability of the facilities was initiated through an anecdote by Lex Smits in which a NASA facility was taken away at the last minute from a carefully prearranged project due to a sudden need within the newly established NASP program, illustrating problems of uncertainty in the use of NASA facilities for fundamental research. After pointing out that "even NASA people have trouble using NASA facilities (it's a hard life!)," Bushnell suggested that arrangements for the use of NASA tunnels be made through on-site "brokers" at the relevant NASA facility. It was suggested that this approach is most useful in establishing ground rules, availability, and other logistical details. Because NASA tunnels operate on overhead, the use of NASA facilities is free to the investigator if the research is of interest to NASA.

A number of issues regarding availability and use of the CTR numerical data bases were discussed. Jim McMichael asked whether it would be of interest to have published guidelines which indicate what numerical data bases are available at CTR and how one might gain access to the data. At issue here is demand, which in turn depends on availability. It was suggested that this point be brought up with John Lumley, head of the Oversight Committee to CTR. Several related points were made. Bert Hesselink indicated that, although efforts are being made to institutionalize the availability of the large CTR databases, considerable effort will be required. It was suggested, therefore, that a wiser approach might be to formulate such data exchanges on an individual basis. Related to this effort is the important issue of proprietary feelings by the researcher who created the data. Concerns about accuracy and validation of these data leads to the need for thorough testing before the data is made available for general use. Steve Robinson pointed out that many of the currently existing data sets are old, have been thoroughly tested and analyzed, and should be made available to the turbulence community. This latter point was reiterated by others. Related issues include inherent difficulties with the size of the data sets, data format, data type, etc.. It is

desirable, for example, to allow for the extraction of subsets of data from a large data set which can be made available in a variety of readable and transportable formats. Related to this issue is the more global issue of data exchange in general, and a willingness for researchers outside CTR to also make their data available to the community at large.

A final word of caution was again made concerning the (in)accuracy of data which has been placed in a 'pool' for general use and, therefore the inherent need for interaction with the creator(s) of the data.

Working Group on Major Areas in Turbulence

Doyle Knight (Rutgers University):

A small group of us was asked by Lex Smits to discuss the issues of 'major areas in turbulence' or, if you like, 'important topics in turbulence.' As Claude Rains used to say, "the group of conspirators was" Brian Cantwell, myself, John Lumley, Rabi Mehta, Dave Wood and John Wyngaard.

We basically ended up with two conclusions. The first conclusion actually originates with Ian Castro, who was unable to join us for our meeting and who suggested to me and to John Lumley that those topics that were listed in the handout which originated from the request Wednesday afternoon actually fall in to two basic categories. The first category we entitled "Fundamental Questions of Turbulence." These are problems which have rather wide application in turbulence research[^] in aerodynamics, in chemical engineering, in biomedical engineering, and so forth. These are, in some sense, fundamental problems and questions. The second category was the category of 'Important Examples.' We could not agree upon a term and none of us wanted to use the word 'applied.' These are questions, applications and examples, but they are those aspects of turbulence research that benefit from an understanding of the first category, fundamentals.

The group was then asked if they could define what they meant by 'fundamental questions', and we limited ourselves to those lists of topics that were on the two pages returned to you yesterday, and this was an incomplete list. This includes topics involving coherent structures, the definition of turbulence (although there were at least two people in the group who felt that was not necessary to answer in order to proceed), the effects of compressibility; fundamental questions such as what happens when you have a flow field where there are shocklets moving around generating turbulence, entrainment, dynamical process which leads to concentrations of turbulence variables, the fine scale structure and its relationship to the rest of the flow, intermittency, kinematics versus dynamics, local isotropy and homogeneity of the fine scale, and several others. Most of these issues were listed on the two pages that were handed out yesterday under the topics "turbulence dynamics" and "energetics". There was one that was added to Brian Cantwell's suggestion, the issue of Reynolds number invariance. We all apply this yet we do not understand why it is true.

The second category was that of 'important examples' of turbulence and I asked the group if they would be willing to take all the other topics on that two-page list not included in 'fundamental questions', and come up with the short list those problems they felt were more important than the others. I arbitrarily made two decisions in my position as the ad-hoc chairperson of this committee. The first decision was that we would not at all address the issue of the definition of 'important'. Each person may use whatever definition they want. Although this seems like a facetious comment, it is not, because I believe that what is important in one person's view is based on a set of criteria, such things as "Can I get it funded?", "Is this going to actually have something to do with improving the quality of life?", "Am I really interested in it?", and whatever else. We also felt that we could not answer that

question in the time allotted. The second criteria that I arbitrarily applied is that we would only list those items in the category of 'more important problems' if at least two different people sitting around the table would agree. So you at least have to convince one other person. Those turned out to be the following: The category of 'most important problems' and the second category of 'important examples of turbulence' are listed in alphabetical order; there is no attempt to do any finer scale ordering. Combustion and mixing, complex flows, two dimensional versus three dimensional, effects of chemical reaction, extra strain rates and so forth. Control of turbulence which was discussed yesterday at some length by a number of people, compressible turbulent flows: hypersonic boundary layers, hypersonic mixing layers, supersonic aerodynamics. Geophysical fluid turbulence, which is a title Tony Perry thought was better than 'atmospheric turbulence dynamics' because it also includes the key issue oceanographic turbulence. And, finally, turbulence modeling^@that is, the development of a set of constitutive relationships that mimic the actual turbulence in the sense that they provide accurate answers to quantities of interest. Everything else on those two pages came under the list of 'others.' It turns out that there was a very high correlation between the number of times the topic was mentioned and whether it appears in this top list, which is probably not surprising.

FOOTNOTES TO KNIGHT:

[1] (i) Could improved collaboration of theory and experiment simplify the approach? (ii) Can existing techniques be used at high speeds (compressible flows)?

Discussion

Fazle Hussain reiterated a point he made earlier, that the broad nature of the list presented by Doyle Knight demonstrates the characteristic of turbulence as a pervasive field with a wide range of application. Consequently, the study of turbulence deserves to be regarded as a fundamental subject within classical physics. John Wyngaard points out that an adverse side of this pervasiveness of turbulence is a lack of communication among groups of researchers focusing on specialized areas of interest at the exclusion of the other areas in the field. Bill George pointed to a more recent development, of which this workshop was part, in which proponents of different approaches to turbulence research have come together in the same room to discuss the field overall. At the "Whither Turbulence" workshop at Cornell (March 1989) it appeared that a consensus was forming in which all approaches are useful. Bill suggested that this sort of dialogue should continue. Steve Kline proposed that, given the small sample of turbulence researchers at this workshop, the entire turbulence community should be included in a larger survey of what are perceived to be major areas in turbulence research.

Working Group on Instrumentation and Facilities

Ron Adrian (University of Illinois, Urbana-Champaign):

The people who met to discuss instrumentation and facilities were Don Rockwell, John Sullivan, Walt Lempert, Fred Browand, Bert Hesselink, Dave Williams, and myself. We examined a number of questions concerning experimental facilities which I shall now report briefly.

Firstly, why do experiments and what experiments are important in turbulence, especially in view of the success of direct numerical simulation? A brief, but non-exhaustive, list of experiments that we will not be able to simulate numerically but which should be done in the near term includes: rough surfaces and complex geometries, real fluids (i.e., fluids with bubbles or polymer additives) and, of course, large Reynolds numbers flows. The Reynolds number should be large enough to produce an extended logarithmic range and a clear inertial subrange.

What is needed? The committee first discussed the issue of big flow facilities. The general consensus was that such facilities *probably are not needed* at this time. But, like the super-conducting super-collider, if some clear, unequivocally critical experiments could be identified in turbulence, (i.e. experiments that hold great promise in giving insights into well formed questions) such facilities might be needed. Diagnostics, on the other hand, seem to be a much more rewarding enterprise, in the sense that many more people in the community could make use of improved diagnostics, and that these diagnostics can help provide the answers to many of the questions in turbulence that we have been considering for decades. In addition, it is important to realize that diagnostics will also contribute to fluid mechanics in general, and to applied turbulence research as well as fundamental turbulence research.

What is status of diagnostics for flow research? In the area of one-point measurement techniques many techniques are relatively well-developed, but some promising developments remain. For example, Lambertus Hesselink is working on a laser based, one-point vorticity measuring technique. But, generally speaking, one-point measurements are not likely to answer many of the important questions we have about turbulence structure. Multipoint methods offer us the possibility of measuring quantities that were not accessible previously by one-point techniques. Instantaneous fields, correlation tensors, true wavenumber spectra, vorticity fields, and dissipation are all quantities that we know are important and that we need to measure. Now we can. Currently, planar techniques are the most prevalent approach: planar laser induced fluorescence, PIV (particle image velocimetry) and variants thereof, MTV (molecular tracking velocimetry), and Doppler imaging techniques. Planar methods allow us to measure a variety of quantities - velocity, concentration and density among them. These techniques are in various stages of development. Some of them are fairly far along, others need more time, but they are close to the stage where we can start using them in serious experiments. That is a major point. In fact, there are many full scale turbulence experiments under way currently using some of these techniques.

What is needed in the area of multipoint diagnostics? In the planar methods, quite a few existing techniques can be brought to the point where they are commonly available within the next five years. This projection is based on analogy with the history of LDV development. For example, PIV and PLIF are at about the same stage as LDV in the early 1970's: they are techniques that work, and people are doing experiments, but the manufacturers have only just begun to enter the scene. LDV did not make serious contributions to fluid mechanics until instruments became generally available commercially. We are not at that point yet. Furthermore, there are many things to be done yet. We need to improve techniques and validate the techniques we have, and we need to increase accessibility by trying to move from techniques available at a few research labs to availability to most researchers. Improved spatial resolution is very important.

What is important to measure? Everyone identified vorticity, and implicit in that is velocity. Especially for high speed flows, density is important; and for reacting flows, concentration is important. There is a fairly clear division between low speed and high speed types of instrumentation somewhere between 200 and 500 m/s. The particle methods work well below these speeds, and above this range molecular techniques are preferable in principle, because of their ability to follow the flow.

Another issue is the measurement of quantities in 3-D volumes, as opposed to planes. There is very little instrumentation for this purpose available now, although there have been some proof-of-principle demonstrations for certain types of measurement techniques. Multiple view photography, holography, magnetic resonance imaging and tomographic techniques with potential. They will require intensive development because they need specialized expertise and rather expensive hardware. Consequently, these sorts of systems will probably be limited to a small number of laboratories. Furthermore, it is going to take considerable effort to make these systems work in the first place, so we are going to pay a fairly high price for volume measurements. The feeling of the committee was that planar measurements can make a big contribution to turbulent fluids right now, and we should go ahead with it.

How should we develop better diagnostics, that is, what is the preferred mode of operation? The committee did not have a very strong consensus on this point. It was felt that we should continue to support individual investigators in the current mode, but it could be very helpful to have several small centers for development of diagnostics in conjunction with experimental studies of turbulence. It should not be a pure diagnostics effort; it should be coupled with fluid mechanics. One of the reasons for this stipulation is to take advantage of existing expertise in groups around the country. One of the reasons for not having a monolithic center is that multiple centers gives some genetic richness to the idea pool and avoid putting all our efforts in a narrow direction. However, each of these centers do need a critical mass to have sufficient manpower, measurement expertise and, certainly, equipment. In addition to the development of experimental techniques, the centers would also provide education in the use of these techniques and dissemination of the knowledge base. These functions could be provided by visitor programs which might involve short-term visits, as well as direct collaboration with people who come for longer term experiments, and also for short courses. Lastly, the committee observed that manufacturers' involvement would help to generate additional funding and make more developments in instrumentation available to a wider community. Perhaps SBIR's would be appropriate as a means of supporting the development of research instrumentation.

To summarize, a special initiative should be made to support development of advanced multipoint diagnostic methods. The time frame should be something like five years. The costs of equipment in this area are such that a combination of several centers could certainly

absorb 5 to 10 million dollars a year. However, in more realistic terms, very significant advances could be made at lower levels, say one and a half to two million dollars per year as an added increment to the existing effort. The instruments resulting from the initiative should be widely accessible to the turbulence community, either through commercial instrument makers, or through collaborations if the developments result in a large, fixed instrument located at a few laboratories.

Discussion

The philosophical issue of the interplay between instrumentation and scientific progress was a theme brought out explicitly in the discussion. Whereas on the one hand scientific and technological problems drive the researcher to develop better tools, on the other hand "we are what our tools make us" - which is to say, major advances in the technology of instrumentation lead to new scientific questions. It was suggested that such a revolution in the technology of measurement is indeed taking place today with the tremendous increase in the amount of detail and complexity in the data which is being obtained. An objection was raised to the notion that the development of new tools is primarily a consequence of global advances in technology when, by in large, the pacing item tends to be new knowledge and scientific interaction. Consequently, the scientific community must recognize the importance of communication within the scientific community as an important element in the development of new tools. This observation suggests that funding for the advancement of instrumentation might be placed on a broader basis than turbulence or fluid mechanics. An example is an NSF-sponsored workshop in 1986 on the topic of "scientific visualization." This is an area that serves many scientific communities, in a sense, "falling between the cracks." The workshop eventually led to an initiative in "scientific computing." The general topic of instrumentation might require a similar approach. It was pointed out that the commercial development of instrumentation generally proceeds slowly, both because commercial interest typically follows from instrumentation development within the scientific community, and also because commercial companies tend to be very conservative and protective of what they regard as proprietary information.

As in the discussion above, communication and "information exchange" was a central theme running through much of the discussion overall. It was suggested that efforts should be made to unify small instrumentation-oriented centers through exchange of researchers and ideas. For example, the concept of large scale high Reynolds number facilities already exists, to some extent, within the atmospheric turbulence community. Jim McMichael pointed out that AFOSR would be announcing a broad-based SBIR program in FY91 to promote collaboration with industry.

Finally, the additional measurements beyond those addressed by Ron Adrian were brought up in the discussion. It was suggested, for example, that fluctuating pressure be included as a measurement for which methodology should be explored. It was also suggested that, along with an emphasis on planar and volume-measurement methodologies, concurrent emphasis should be placed on advancing the state of the art in single point measurements to include simultaneous multi-directional information, such as the full deformation tensor, so that tensor invariants and simultaneous measurement of different turbulence variables could be obtained. Finally, it was pointed out that advances in surface measurement techniques, often the major focus in practical applications, should not be excluded from consideration.

Working Group on Advocacy

Jim McMichael (AFOSR):

The members of the discussion group were: G.L. Brown, C.H. Ho, F. Hussain, S. Lekoudis, J. McMichael, P. Purtell, A. Roshko, and K. Sreenivasan. I am reporting on this, more or less, as a scribe or secretary and at the conclusion of this summary we can discuss the issue in more detail and try to respond together. Let me also say that I think Dave Dolling's summary this morning was excellent. In fact, he already said much of what I have to say. Perhaps the clarity of his summary indicates the ability of an individual to summarize more effectively than a committee. We considered three major questions: What are the advocacy arguments, who should make them, and to whom should they be made? We talked a little about the context, or scope of the discussion we should have--whether we should address only aeronautics or whether we should put this in a larger context. However, we did not feel that we necessarily represented the larger context very well and perhaps we ought to talk about advocacy more or less within the context of aeronautics.

We identified two fairly obvious objectives for advocacy. One is increased support, or at least to maintain adequate support for some of the important things that we feel we ought to do as a community. Also, there is value in advocating to attract talent to the field--bringing in bright new students or people from other disciplines who have special expertise. We decided that advocacy falls into two categories: long term and, I actually like the term that Dave used this morning a little better than "short term," that is, medium term. The long term arguments would be focussed more towards developing an overall awareness and a receptivity among people whose decisions affect us in the long term. Short term or medium term advocacy appears to amount to something like lobbying, although we did not reach a consensus on that. The discussions were quite strained as to what ought to be done in terms of nearer term, focused advocacy, and in some sense perhaps we could say that the discussions stalled out. What emerged was a feeling that the community should proceed gently and thoughtfully in that area with perhaps a pilot project to start.

What are the advocacy arguments? Technological relevance can be discussed in terms of payoffs, or at least the potential for success. We did not think it was necessary that we be able to promise to deliver specific payoffs, but at least we have to show that we have potential for delivering them--that there is good reason to think we could do something now that we could not do some time ago. The key to this, of course, is new ideas. The whole advocacy concept ultimately rests on the continuous emergence of new ideas, and new developments which allow us to do things differently than we could in the past. We also worried a little about the down side--the consequences of failure if one promised a very specific deliverable, and it didn't work out. What would the consequences of that be? Obviously, we need to think carefully about this.

Examples of advocacy arguments can be found in documents such as the DoD "Critical Technologies" list. There is a discussion in there about how turbulence affects CFD for example. The basic notion is that turbulence models are a pacing item for CFD. There are

also other particular technological issues that could serve as a focus for discussions of technological importance; engines and air breathing propulsion were mentioned by Gary Brown yesterday.

The issue of who should make the arguments was also discussed. We can see arguments being made at many levels and I think Dennis Bushnell summarized it well by saying that advocacy is a job for "everybody, all the time, everywhere". Certainly individuals can advocate. Individuals can also encourage professional societies to make some of these arguments. Perhaps even the national academies can be involved in making some assessment of the needs for turbulence research. We also talked a little bit about the potential value of having a third party, industry for example, express the need for turbulence research.

We talked about the possibility of forming an advocacy group for turbulence. Sreenivasan read from a draft letter about a proposed advocacy group yesterday, but that was a little too quick for us to digest. The issue that we talked about last night was that, if there were such an advocacy group, who would it be representing and how would they assume some sense of legitimacy? One possibility is for the group to be endorsed by societies. On the other hand, perhaps it would just be an ad-hoc collection of interested scientists. We didn't resolve that. In fact, we thought that perhaps this is an issue we should set aside for the moment and look more at what such an advocacy group might do.

To whom should the arguments be made? Well, again as Dennis said, to our colleagues, perhaps professionals in other related fields, to students certainly, to the public at large, and certainly to agency program managers. I think this last point is important; I can identify with this point very directly. Program managers have the responsibility of advocating within their agencies. However, not all agencies are represented here at this meeting and perhaps one could talk with some of the other agencies such as NSF, DOE, EPA and others to help them advocate turbulence within their own organizations. Should advocacy be taken to higher levels? Yes, although we did not identify as clearly where advocacy arguments might be made at higher levels. Again, I think the general consensus was that this has to be done gently and very thoughtfully. Congress?--well, the group felt that was the last level to think about, and there we have to be particularly careful, particularly gentle. There, perhaps, the arguments need to be made within the context of national priorities. The question is "how do you advocate within the groups that are dealing with the national priority issues such as energy and the environment?"

We can point to some successes of past advocacy. The heat transfer community has organized itself around an ASME group which has worked on this problem. They actually did get industry involved in making third party arguments in support of the engineering community and I think they have had some success in getting some support from NSF. Low temperature physics has evidently also had some success with NSF. The plasma community has had some success; they are driven by what is clearly a national issue--energy and fusion. The propulsion community is also similarly concerned about advocating their case. That group is actively engaged in the same kinds of discussions; they may be a little bit ahead of us, in fact. The sense I get, from what I know of it is that their effort is primarily directed towards a small sustained broad-based advocacy.

Action items. We had difficulty settling on much in the way of definite action items. One thing we did agree on is that the ad-hoc letter should be sent to some people in the community, absent the discussion of how you should decide who should be on the committee, but that a few names of people who might be on such a committee should be suggested and other names should be requested. We thought that maybe there would be some value in

seeing what the response to that letter might be. Possible actions, as I mentioned earlier, may be a small pilot group, two or three individuals, as interested concerned scientists, might carry particular arguments to higher levels perhaps in some of the agencies as a sort of learning experience. Overall, our advice to the community is that this should be done rather gently and thoughtfully.

We talked a great deal about the technological relevance of what we do. The key to new ideas is often basic scientific curiosity, hopefully stimulated by an awareness of the issues of technological importance. But in any case, a healthy research environment requires continual injection of new ideas. Then perhaps there is also a need to address the attitude of industry toward the value of basic research. If advocacy arguments are made, one wants implicit industry support. One does not want to have the people to whom the arguments are made to go out to industry and other places and hear less than enthusiastic support from our user community.

This gives you the flavor of how the discussion went. We will all be happy to respond to any questions or discussion points you want to raise.

Discussion

From the recorder of this discussion: Because of the nature and subject of this discussion, I felt that the spirit of the discussion would be better preserved if the comments were written down in the order in which they were made. I have taken the liberty, however, to edit the remarks to reduce their length and (hopefully) improve their readability while retaining the content and spirit of the points being made.

F. Hussain: I wish to make the point that, because it is an important issue, the question of "advocacy" should be shared with the larger community. This workshop is not a very large community. There is some concern that, before this issue is taken further, the community must give its support and legitimacy, and must decide what mechanisms are appropriate to make the effort effective. If this is not done, it is unlikely that a similar effort will be launched again.

D. Knight: I continue to wonder why there is the need to establish yet another committee to advocate additional funding in fluid mechanics or turbulence? I belong to many professional societies with offices in Washington which understand how Congress and the federal agencies operate. Why, for example, has the Division of Fluid Dynamics of the American Physical Society (APS/DFD) not been asked to lead this effort on our behalf in Washington?

F. Hussain: Turbulence belongs to many communities, from astrophysicists to geophysicists to biologists to mechanical engineers. This unique characteristic of the field makes it very difficult to obtain strong support from one single organization; nor would that one organization be a proper representative of the diverse nature of the field. Precisely for that reason I see the need for an advocacy group.

S. Kline: Let me expand on this point a bit. We are passing out of an era where everything that we would like to have funded can be funded. A couple years ago a Japanese official asked "why is it that after World War II the United States enabled science but neglected its technology?" There are some new initiatives on technology policy being considered, and things are not going to be as they have been. We can associate the need for advocacy within the turbulence community with these issues.

K. Ghia: I agree that there are diversified areas in which turbulence is being pursued. On the other hand, most of the researchers who measure turbulence belong to APS, AIAA, or ASME, so I wonder why representatives from these groups are not part of this effort? Furthermore if biologists and others are important to the "advocacy" question, why was only a very select sample of researchers part of the discussion?

F. Hussain: This goes to the heart of the comment that Jim (McMichael) just mentioned (about the ability of a group versus an individual to make decisions). Yesterday, when our group met to discuss again the formation of an advocacy group for turbulence, we identified over twenty different research communities where turbulence plays a role. It would be difficult to run an advocacy group if a token representative from each group were included - that means only one member from the aerodynamics community! The purpose of our meeting was simply preliminary discussions to get the dialogue going. It did not represent whatsoever the different constituency groups. It was not the intention that this group be the advocacy group, but rather an effort to discuss the possibility of launching such an advocacy group for which the community at large should be approached. I should also mention that similar meetings were also held in Japan and in the Soviet Union, where worldwide support for turbulence research was discussed.

A. Smits: Kharman (Ghia), you were largely responsible for putting together the first National Fluid Dynamics Congress. At the time, many people were asking "why do we need another conference?" But I think the National Fluid Dynamics Congress was actually an attempt to bring some of these communities together with a focus on fluid dynamics. I think that this is another part of the advocacy process, to identify the community by pointing to a national conference - like the National Heat Transfer Conference.

S. Kline: There is a focus for heat transfer in a single society. Heat transfer in ASME has always been the focus. But this community has never had such a focus.

K. Ghia: Yes, ASME heat transfer has provided a focus, but AIAA is equally alive. What Lex (Smits) pointed out about the National Fluid Dynamics Congress is true. However, in the organization of the Congress we reached out and had representation of every society on that committee before we embarked on the project. This was my point. I was hoping that prominent turbulence researchers from other areas were invited, so that the advocacy process would be initiated in the right way.

V. Kibbens: I wish to bring up a somewhat different point. A recent major success that could be claimed by the field of turbulence research is wind shear detection. This topic hit the public conscience in a big way and loosened many purses. It falls in the category of something that evokes the "boundary conditions" of human existence in a very personal way, as does particle physics and cosmology. In plotting the strategy for advocacy we could think in terms of issues that have this kind of feature. Examples might include the environment, weather, microbursts and global heating.

G. Brown: Personally, it has been very encouraging to see a level of consensus develop about important problems, and to see that we are poised to make major progress for a number of identifiable reasons. I am in favor of some sort of "pilot process" where, with some broad consensus from this meeting, we approach the National Center for Advanced Technology of AIAA, which has the specific objective to act as an advocate and to build bridges between communities, and to ensure necessary funding. What is being discussed here is something which is certainly industry driven, and partly defense driven. Approaching the National

Center for Advanced Technology is a reasonable first step for a small group, which would then express the outcome in a letter, for example, to those who attended this meeting.

G. Settles: Perhaps it is time for a blatantly self serving popular article on turbulence in Scientific American!

L. Hesselink: Possibly another way for approaching advocacy is to simply speak up for one's own interests. This was recognized, for example, at the National Science Foundation where a program was introduced whereby one can spend up to a year doing "technology transfer." Much support exists for good ideas which can be used by industry. It might be worthwhile to contemplate spending paid time to advocate one's own ideas either in industry or in government.

F. Hussain: The question of individual advocacy was discussed both times we met. The role of the advocacy group would be to facilitate personal advocacy and not to isolate individuals. However, in situations such as critical defense policy, where individuals do not have access to the decision-making process, there is a legitimate need for group advocacy.

Regarding the National Fluid Dynamics Congress, my perception is that this effort is not as broad as turbulence is, in that this is a more applied group than APS/DFD - which contains a large fundamental science component. Furthermore, astrophysicists, meteorologists, oceanographers, etc. were not involved. I know of no single organization with the breadth required for turbulence advocacy, and I do not see where we are duplicating another organization or an alternative format.

F. Browand: Gary Brown's comments suggest that he would like some sort of answer from us: that he would be interested in organizing the effort he mentioned. Do you want a response from us, and perhaps make your proposal a little more definite?

G. Brown: I think what I proposed is a realistic outcome of this meeting - to say to the National Center for Advanced Technology that we are concerned about the way the funding is going - and I am happy to express that view with a mandate from a meeting such as this. I would simply say that we had this meeting and that the our first concern is to create a larger awareness amongst the professional communities.

S. Kline: Would you like to see a show of hands in favor of your doing that?

G. Brown: O.K.

S. Kline: How many here feel that what Gary Brown is proposing would be useful? (perhaps 60-70% responded favorably). There was a suggestion that a letter would be sent out to the turbulence community. Perhaps you should get together to discuss this further.

F. Hussain: The letter will be sent out; however, this straw vote could perhaps be interpreted as another basis for legitimacy.

D. Bushnell: I was taught many years ago that this is a democracy and that the essence of a democracy is that there are many buttons that must be pushed to get anything done. Yes, do the letter. Yes, Gary Brown should go to the National Center for Advanced Technology. Yes, do it! Do whatever it takes.

S. Kline: I think this is a good place to stop.

Working Group on Junior Faculty

Tim Wei (Rutgers), Eric Spina (Syracuse), Amy Alving (U. Minn), and Hussein Hussein (George Washington):

This position paper was written to address the need for developing and training new university faculty in the area of experimental fluid dynamics. As representatives of the senior experimental fluid dynamicists of the next century, we have several concerns which bear on the long-term health of our discipline. It should be noted at the outset that our field is currently fortunate to have a balanced distribution of members: from senior leaders to junior faculty. However, the increasing costs of experimental research combined with reductions in research funding threaten this delicate balance. The community is very sensitive to small changes because of the small number of academic experimentalists; we can ill afford to reduce the pool of new members. At the same time, few people would dispute the important role that experimentalists must play to make progress in the field of turbulence research. Thus, to endure the continued health and vitality of the turbulence community, we must engage in an effort to properly train new and existing junior faculty in the field of experimental fluid dynamics.

Clearly, the time to develop the future leaders of the turbulence community is now, while there are still a significant number of experienced scientists to act as role models and mentors. We strongly feel that the training to be successful researchers, and more importantly, to be responsible leaders of the turbulence research community, does not end with the granting of a Ph.D. (or with the completion of a post-doctoral appointment). Rather, the training must continue throughout the first five to ten years of an academic career, and should consist of a range of support, including mentoring, inclusion in technical groups and committees, and some special consideration of a junior faculty's position. This is a complex problem requiring significant contributions from a variety of sources including: the government, the universities, the turbulence research community, and the junior faculty themselves. The contribution that each sector can make to the development of future leaders of the turbulence community and to the concomitant development of the following generation of engineers is discussed below.

Contributions of the Junior Faculty

Ultimately, we, as representatives of the junior faculty of 1991, recognize that our success is our own responsibility. Our primary objective in the early years of our academic careers is to develop the skills necessary both to train the engineers of the future and to provide technical leadership for the nation. As part of this development process we must:

Learn to identify fundamental research problems relevant to the major engineering and societal issues of the day (and not merely make incremental improvement on our Ph.D. theses),

Establish focused research programs to address these important issues,

Expand our knowledge base and increase our understanding of turbulent flows to gain a broader perspective on the field of turbulence, and

Develop effective teaching skills required to train the students of tomorrow.

These four tasks require that we seek external funding for our research programs, teach a variety of courses in our discipline, publish scholarly papers of high quality at regular intervals, and attend scientific meetings.

Contributions of the University Administration

The university has a vested interest in the success of its junior faculty. The future strength and reputation of the university is entirely dependent on its young educators. In this time of shrinking R & D dollars, the university administration can assist junior faculty with tangible support as well as simply recognizing the difficulties of establishing an experimental research program. The net production of an experimentalist after six years in academia simply cannot be measured on the same scale as that of other disciplines. Specific suggestions to the university administrators regarding the development of young experimentalists include:

Provide sufficient start-up funds to enable an experimentalist to develop a competitive research program in a specific sub-discipline of turbulence;

Realize that experimental fluid mechanics research (and turbulence research in particular) requires significant resources, and is not a field where a one-time resource infusion is sufficient to ensure the success of a long-term research program;

Train junior faculty in the intricacies of acquiring research funding, including the developmental ties with funding agencies, the marketing of research ideas, and the submission of proposals. In particular, during this period of reduced federal funding the university should promote relationships between junior faculty and local and state industry;

Reduce the level of indirect costs charged to granting agencies in the research proposals of junior faculty whenever feasible, and

Place greater emphasis on the quality of scholarly activity rather than the quantity during tenure review. The current tenure system rewards junior faculty for making incremental advances in an area rather than developing a comprehensive research program to make significant contributions.

Contributions of University Departments

The department and its senior faculty must also play a major role in the development of the junior faculty. The support of the fluid mechanics faculty is especially important, since they know better than the others the tasks faced by experimental fluid dynamicists. Some support offered to junior faculty may adversely effect the senior faculty to some extent (technician time, teaching loads, etc.), but support early in an academic year will decrease the timeframe in which the junior faculty can make a significant contribution to the department and to the field. Senior faculty could:

Propose a slight redistribution of some departmental resources towards junior faculty (this includes technician time, teaching assistantships, etc.),

Help in the identification and pursuit of funded research opportunities,

Offer advice in the maintenance of a proper balance between teaching, research, and university service (note that this includes criticism as well as praise in yearly tenure reviews),

Help to ensure that, at very least, the junior faculty are not forced to teach different courses each year,

Promote work of junior faculty at meetings and workshops and to industrial contacts, and

Encourage that travel funds be set aside for junior faculty to attend important professional meetings (even if they will not be presenting a paper).

Contributions of the Government

The federal (and state) government has two primary missions with regard to science and technology. The first mission is to fund research which addresses today's technological problems. The second mission is to ensure the development of a sound technological base to adequately address the critical problems of tomorrow. Inherent to the second mission is the training and development of young faculty to be the researchers, educators, and leaders of the future. Because government research programs must satisfy the first mission (immediate results) to justify their existence, there can be a tendency to neglect the second mission. Government funding agencies must realize, however, that the investment in the research of young faculty will pay long-term dividends, and the often contradictory goals of the two missions must both be satisfied.

Experimentalists as a group are perhaps the most sensitive to how well the government accomplishes the second mission. The costs are higher and the rate of return tends to be lower in experimental research than in computational or theoretical research. It is critical that government funding agencies recognize these "facts of experimental life" and the important role which experimentation still plays for physical insight and model verification. In response to these facts, the government ensure adequate funding is available for young experimentalists to establish research programs and to develop as successful researchers. An important assist can be supplied to young experimentalists by:

Expanding the opportunities available to engage in significant collaboration with government laboratories,

Creating junior faculty award programs targeted solely for experimental research,

Reviewing existing young investigator programs to ensure that selection criteria are not biased towards one particular group, and

Notifying qualified junior faculty of upcoming initiative and encouraging their participation in that area.

More generally, we encourage the funding agencies to expand graduate fellowship opportunities in engineering. While the NSF and NDSEG fellowship programs are well-intentioned, they do not go far enough. A broad expansion of these engineering fellowship programs, could be an effective way to combat several weaknesses in the U.S. engineering infrastructure. An increase in the number of available fellowships would: encourage more of the nation's best undergraduate engineering students to pursue advanced degrees (the current fellowship programs are too exclusive to encourage any students but the very best from the leading universities); create a larger pool of candidates for junior faculty positions, thereby increasing the quality of future academic leaders; and aid young faculty, whose ability to provide support for top graduate students is uncertain.

Contributions of the Research Community

The turbulence research community also has a vested interest in the success of its young members. A community which promotes young membership ensures its continuation and creates an image of vitality which is important in its success. In particular, vitality and growth will be the key factors in whether the turbulence community is appointed its share of available resources. Steps that the research community can take to promote and develop its junior membership include:

The development of a mentoring relationship between each young faculty member and an established researcher in the same area,

Inviting young faculty to attend and participate in workshops, and

Inviting young faculty to observe and participate in technical service committees.

Conclusion

As evident from the discussion above, the development of young faculty is important to a range of people and institutions. There are benefits at every level, from society as a whole down to individual engineering students. Society will profit from the development of technical leaders, the university gains through the enhancement of its reputation, students benefiting by being taught by a young active researcher, and the young faculty member certainly benefits from the flourishing of a career.

One of the issues raised in this workshop was the recruitment of young people into the field of fluid dynamics, and turbulence in particular. If an environment is created where a young faculty member is carefully groomed and encouraged to develop a rewarding research program, there is little doubt that young, scientifically-oriented people will be attracted to the field of turbulence. This is a critical time in the field of turbulence as well as in this country. We, the young faculty, are willing to do what we can to contribute to the advancement of our field and the furtherance of its goals. However, we need the recognition and help of our research community, the federal government, and the universities to effectively fulfill our role.

SESSION 7

The Future

Session Chairman: B. Cantwell, Stanford University

Session Recorder: J. G. Brasseur, Pennsylvania State University

John Wyngaard (NCAR/ Pennsylvania State University):

I want to thank Lex Smits for asking me to talk about the future. I do not have a crystal ball, but I do work in a different community, geophysical turbulence, so my perspective is different from yours, and I hope of some interest.

As I reflect on the week and think about the future, I remember some statements from the speakers. "Turbulence is a serious technical problem, yet has virtually no national profile." "We need a sense of community." "If you can compute, do so, because there are things we cannot measure." "Theory gives meaning to what we do." "Some good bets for future research are complex stratified flows, particles, phase change, latent and sensible heat transfer." "Experiment is our most important activity." "We need large national facilities." "It is difficult to find students." "We need to deliver increased understanding and better models."

These are all good points that bear equally well on geophysical turbulence. There is a long tradition of geophysical turbulence research, starting with the two-dimensional turbulence of the general circulation, predictability, and standing eddies. But three-dimensional turbulence in the upper ocean and the lower atmosphere has a long tradition also and today, more than ever, I think there is a strong need for improved understanding and better models of geophysical turbulence. Why? Because, as a few of you pointed out, this is the age of global-change research. A few months ago, Prime Minister Thatcher announced the formation of the Hadley Center for Global Change Research. It is headed by a geophysical turbulence researcher, David Carson. In the U.S., there is lobbying for a climate systems modeling project with a dedicated Cray computer.

A principal vehicle for this global change research that continues to make front-page headlines is numerical modeling of the global climate system. These models need representations - parameterizations, or sub-models - of turbulence effects in the upper ocean, in the lower atmosphere and in clouds. These parameterizations, to put it kindly, need work. I do not want to mislead you; this is not seen today as the global change community's highest priority. But, on the other hand, they would not be unreceptive to proposals for systematic and serious work on improved turbulence models. We have developed an approach to this problem; it is based on large-eddy simulation. But we have developed this approach, to

a large extent, in isolation from your community.

This brings me to my first recommendation for the future. We need bridges between you and us. Bridges in academic institutions - students, post-docs, visiting professors; bridges in research institutions - joint projects, communication. In federal agencies and in professional societies I do see encouraging signs that bridges are being built. There is increased coordination, joint funding, and the like. We need more.

But geophysical turbulence is different from your turbulence. How? Well, most conspicuously, but perhaps least importantly, it has huge Reynolds numbers. Re 's of 10^{14} are routine. I think what is more important is that our turbulence tends to be either stably or unstably stratified. We have developed different models, different closures, different parameterization statements - because of the real differences in our turbulence. This brings me to my second recommendation for the future. If in fact, we push for large national experimental facilities, let them be able to incorporate buoyancy effects, so their results have some geophysical relevance.

Finally, as I said, we do regard LES as trustworthy in geophysical applications. It has its problems, to be sure. In fact, I like to say it is the worst form of geophysical turbulence modeling, except for all the others that we have tried. I would stress two points here. First, we have a much more difficult time than you in making observations. Second, often we need to know the behavior of turbulence systems. For example, take severe storms. There have been extraordinary advances in our understanding of severe convective storms, including some very perceptive insights into the formation of tornadoes, through large eddy simulation. This work was summarized by Joe Klemp of NCAR in a recent edition of Annual Reviews of Fluid Mechanics. But we also use LES for studies of turbulent processes for example, the transport of passive conservative scalars by turbulent convection. That is a problem that has yielded quite nicely to LES. In summary, my recommendation three, and this does parallel some earlier recommendations, is that we need to increase the accessibility of DNS and LES so that turbulence simulation can really reach its potential. It is today a very high overhead tool.

In summary, we need bridges between geophysical and your traditional engineering based turbulence community. If we push for experimental facilities, let us make them very general - large Reynolds number, but capable of simulating buoyancy-driven or buoyancy-suppressed flows. And, finally, let us reduce the overhead of turbulence simulation so it can really achieve the revolution that we can all see coming.

Final Discussion

Three areas of discussion were brought up: (1) availability and uncertainty of DNS (and other) data bases, (2) the issue of a large Reynolds number facility, and (3) comments on "building bridges" and the manner in which turbulence research should be carried out in the future.

(1) Considerable discussion took place around the issue of DNS availability and uncertainty. Bill George opened the discussion by pointing out, with reference to the resistance to making numerical data available on a large scale because of concerns of accuracy, that the approach taken with experimental data has traditionally been to repeat the experiment in other places and "fight it out." "Why this fear?" The suggestion was made that the DNS data base should be organized and made available for different researchers to use in different ways, but that the problem of data accuracy is an important one, requiring some kind of uncertainty analysis which is seldom done. It was pointed out that the standard method of reducing the mesh size as a measure of discretization error does not work with DNS of turbulent flows because the computer is generally strained to the limit to produce the highest possible Reynolds number. A method developed by Joel Ferziger based on Richardson extrapolation was mentioned as potentially useful here, but some measure of phase and dispersion error should also be provided. The point was raised that, no matter what the source of the data, a major source of uncertainty which is generally not documented is often the boundary conditions, or other flow conditions. The issue of uncertainty, in general, is not an easy one. However, there was universal agreement that, regardless of the difficulties, all possible information relevant to uncertainty should be provided.

(2) The issue of a large Reynolds number facility was again brought up when Tony Perry pointed out that the cost of such a facility is really not so great when one considers that a billion dollar facility which produces a Reynolds number of a billion "works out to only one dollar per unit Reynolds number!" He suggested that support might be solicited, not only from the United States, but also from Europe or Japan. Cantwell argued that some high Reynolds number flows are of sufficient importance to the aircraft industry to suggest the possibility of significant industry support for a high Reynolds number facility. A heavily instrumented aircraft might also be useful approach to the measurement of high Reynolds number flows; such a concept had been seriously discussed in the past at Boeing.

(3) In response to the request by John Wyngaard for a high Reynolds number facility that can also simulate geophysical flows, Fazle Hussain cautioned that a facility designed to serve multiple purposes can sometimes lead to a compromise in quality in the area for which the facility was primarily intended. Wygnanski pointed out that the choice and application of "generic problems" at the heart of turbulence research should be expanded to include related areas. An example is the generic issue of two phase shear flow which has wide applicability, but to which the classical single phase mixing layer studies are not sufficiently useful. Wygnanski further argues that closer links between direct numerical simulations and experiment should be encouraged in collaborative research projects between simulators and experimenters.

Anatol Roshko ended the meeting by reminding us all that

"Turbulence is both a beautiful and a frustrating discipline."

APPENDIX A

The Role of Experiments in the Study of Fluid Mechanics

(Plenary Lecture, Second World Conference on Experimental Heat Transfer, Fluid Mechanics and Thermodynamics, Dubrovnik, Yugoslavia, June 23-28, 1991. Presented by A. J. Smits, Department of Mechanical and Aerospace Engineering, Princeton University, Princeton NJ 08544, U.S.A.)

Abstract

The role of experiments in basic fluid mechanics research is considered, especially in the light of recent developments in computer technology and new instrumentation. Future directions for research and strategies for future development are also discussed.

Introduction

As recently as ten or fifteen years ago, there was little question regarding the role of experiments in fundamental fluid mechanics research. Almost all practical problems, especially those problems where the flow was turbulent, could not be treated analytically without broad simplifications, and computers were not nearly powerful enough in speed or memory capacity to deal with the exact equations. Calculations were based on the Reynolds-averaged equations of motion, virtually without exception, and although turbulence theories were important in the development of turbulence models, no significant progress could be made without experiments. Experiments were essential to improve the fundamental understanding of turbulence and to provide the statistical data necessary to improve turbulence models. There were no alternatives available.

It could be said that experiments were then providing the most significant insights into the behavior of complex fluid flow problems. Of course, analytical and theoretical work were also making important contributions: achievements such as the analysis of the stability of laminar flows to small perturbations [1], the analysis of isotropic, homogeneous turbulence [2] [3], and the development of stochastic tools for higher-order turbulence modelling [4] are major milestones in fluid mechanics. Turbulence, however, was always approached as a problem in statistics. "As a consequence of the irregularity and complexity of the motion, it is only practicable to consider mean values of functions of the instantaneous and local values of the fluid velocities and pressures, and all theoretical and experimental work uses mean values" [5]. Yet it was widely known, through experiment, that turbulence displayed deterministic features as well. Experimental observations of phenomena such as the non-linear stages of transition [6], the appearance of turbulent spots [7], the turbulent bursting cycle in wall-bounded flows [8], and the persistence of organized large-scale motions in turbulent mixing layers [9], and boundary layers [10] had a radical influence on the field of turbulence. They had a profound effect on how turbulence was perceived, how it should be modelled, and how it might be controlled.

A turning point in changing the role played by experiments came during the 1980-81

Stanford Conference on Complex Turbulent Flows [11]. The conference was marked by two major events. First, it was concluded that calculations based on the time-averaged form of the equations of motion were now capable of dealing with considerable geometric complexity, but that turbulence modelling was still fraught with uncertainty. Any calculation method which was used to predict the behavior of a flow outside the regimes where experimental data were already available would require further experimental work to validate the results. Second, the power of Large Eddy Simulations (LES) was demonstrated spectacularly in a flow of great engineering interest. Moin and Kim [12] presented an enormously impressive computation of a fully-developed channel flow, where modelling was only used to represent the sub-grid scales (thereby adapting a technique first used by Deardorff [13]). The calculation used the world's first supercomputer, the ILLIAC IV, and three-dimensional graphics were used in a brilliant demonstration of the detailed turbulence behavior. It was obvious that physical experiments were no longer the only tool for studying turbulence behavior, but that numerical experiments had suddenly come of age.

The explosive increase in computer speed and memory capacity, and the development of three-dimensional color graphics workstations in the past ten years has made possible the development of Computational Fluid Dynamics (CFD), an entirely new field of study. Computations have already moved beyond LES to DNS (Direct Numerical Simulation), where no turbulence modeling is used at all. The most impressive DNS contribution to date is without doubt the calculation of a low Reynolds number turbulent boundary layer by Spalart [14], and its interpretation by Robinson and Kim [15]. Here, we have a calculation more detailed than any experiment currently available, making it possible to study features which are usually inaccessible to measurement, such as the time-dependent, three-dimensional velocity and vorticity field, its corresponding pressure field, the instantaneous Reynolds stress distributions, and the instantaneous dissipation field. Another example of the power of CFD, this time in the area of aerodynamics, is the representation of the inviscid, compressible flow field over an entire aircraft by Jameson, Baker, and Weatherall [16].

Where does that leave the role of experiments? If some voices are to be believed, it is now possible to do everything on a computer, or at least it soon will be. What will still be worth doing experimentally in 10 or 20 years' time? Will it be true that just a modest breakthrough in sub-grid-scale models, to make LES at high Reynolds number trustworthy, and the floodgates are open? Is the declining number of experimental studies reflecting the power of the computer to solve a wider scope of problems? Are experiments too expensive? Too slow? Why does it seem more difficult to attract good students into experimental work? Is experimental work headed for obsolescence? If we take the papers presented annually at the meetings of the APS Division of Fluid Dynamics, 44% of the papers were experimental in nature in 1981, compared with 36% in 1990. Not a huge decline, but is it indicative of a long-term trend?

It was at least partly with these questions in mind that a workshop on "New Approaches to Experimental Turbulence Research", subtitled "Experimental Turbulence Research in the 21st Century", was held at Princeton on September 5-7, 1990. The focus of the workshop was to address some issues facing the experimental turbulence research community, such as the question of its relevance to advances in fluid mechanics, the role of computers and instrumentation, funding sources, education, and faculty development. It was hoped that the major concerns facing the community could be identified, and that we might develop a strategy to guide our future activities. About 50 research workers in turbulence attended, from all aspects of turbulence research, over a period of two and a half days. In this paper I will try to analyze the results of that meeting and to discuss some other areas of interest to those of us who still believe that experimental research in fluid mechanics is important, and

who feel that experimentalists are in danger of becoming an endangered species.

Developments in Experimental Techniques

Whereas the rise of CFD has been widely acclaimed, it has not often been recognized that major changes have also been occurring in experimental fluid mechanics. While the computer was making an enormous impact on the possibilities for computing flow fields, it had an equally important impact on the possibilities for experimental work. Furthermore, the widespread availability of lasers, over a large range of wavelengths, also had a major influence on the quality and scope of the data obtained by experiment, especially when used together with sophisticated computer analysis of the data.

Computers made their first impact on experimental work about twenty years ago when it became routine to digitize hot-wire data. This made it possible to perform extensive analysis of the time-varying signal, to use multiple probes, and to examine interesting portions of the signals in great detail (see, for example, Blackwelder and Kaplan [17]). On their own, or when combined with simultaneous laser-sheet flow visualization (Falco [18]), these observations led to a completely new interpretation of wall-bounded turbulence. The relatively recent development of very high-frequency data acquisition systems have allowed similar data to be obtained in high Reynolds number, supersonic flows [19] [20]. Since the introduction of the laser-Doppler velocimeter in 1964 by Yeh and Cummins [21] great progress has been made in applying lasers to quantitative measurements of turbulence, especially in the area of combustor flows, where Raman scattering, CARS (Coherent Anti-Raman Scattering), and Rayleigh scattering techniques have been used to measure density, temperature, velocity, and species concentration (for reviews, see [22] [23]). Laser-Induced Fluorescence (LIF) techniques have also been used to obtain temperature, density and pressure data in turbulent flows [24]-[27], and they can be used to give instantaneous distributions in a plane [28]. Three-dimensional scalar fields may also be obtained using LIF [29], and Rayleigh scattering techniques have been used in supersonic flows to image the instantaneous planar density field [30]. Another powerful new technique is PIV (Particle Imaging Velocimetry), based on the three-dimensional imaging of a large number of small particles [31], [32]. In such experiments, the density of the data sets approaches that of a typical DNS computation. For instance, in the three-dimensional, time-evolving visualization of a smoke-filled, low Reynolds number turbulent boundary layer [33], 96 time steps were recorded, each containing 2×10^6 pieces of 8-bit data. Here, the vast data sets generated by large-scale computations and detailed experiments confront a common problem: how to represent and display these data sets so that some interpretation is possible. Image processing is widely used, and many different approaches have been suggested (stereo pairs [34], transparency plus motion [35], etc.). For a review see Hesselink [36].

Relevance

Consider the three-dimensional, time-dependent DNS computation of a low Reynolds number turbulent boundary layer by Spalart [14], and the three-dimensional, time-dependent smoke-flow visualization of the same flow by Goldstein and Smits [33]. The fact that the Reynolds number was low in both cases allowed an adequate resolution, with a reasonable time step, in both the experiment and the computation. So why bother doing the experiment? The computation will give the three-dimensional velocity field as a function of time, whereas the experiment will only give, at best, the scalar concentration field. Even if the experiment was repeated using the PIV technique, the information content could only match the

computation, not exceed it. Some reasons can be offered:

1. The computation needs experimental validation. This aspect of DNS is sometimes overlooked. The computations are limited in spatial resolution, and organized motions can occur on the sub-grid scale (see, for example, [37]). Furthermore, the use of periodic boundary conditions is physically unrealistic, and it requires the use of a fictitious external force to produce a temporal boundary layer growth.
2. Particle dispersion can be studied easily in the experiment, whereas computations are invariably based on an Eulerian representation, and Lagrangian statistics can only be obtained at a very high cost.
3. The experiment is cheaper (a fact often ignored in a world where institutions will assume the cost of a computer, much as they do a library facility, and not assume the cost of an experimental facility). The study of many time steps becomes possible, thereby allowing some statistical analysis on the significance of features observed in the flow. In contrast, calculating and storing intermediate time steps in a computation can be prohibitively expensive.
4. Low Reynolds number flows can be used as a testing ground to develop experimental techniques which can be used to examine higher Reynolds number flows. For example, an experiment to obtain the full three-dimensional flow field at low Reynolds number can be used to determine two-dimensional cuts which are, by some criterion, the most revealing. Then the two-dimensional cuts can be obtained at high Reynolds number, and still provide a basis for comparison with the low Reynolds number results.

Some of these reasons are more compelling than others. As computers increase their capability, reasons 1 and 3 may vanish more quickly than 2 and 4. Despite these limitations, it seems likely that within the next ten years the DNS approach will furnish us with a very rich and reliable data base for the understanding of a wide variety of interesting turbulent flows. These will probably include spatially developing flows such as low Reynolds number boundary layers, mixing layers at slightly higher Reynolds numbers, and fully-developed pipe and channel flows at even higher Reynolds numbers. Heat transfer information can be produced with only a modest additional overhead, and it should be possible to study the effects of compressibility, at least at low Reynolds numbers. Where are the limits of DNS?

1. The CPU requirements are proportional to Reynolds number cubed, and the storage requirements increase as Reynolds number to the power $9/4$ [38]. Together with a reasonably optimistic estimate of growth in computational power, a factor of 10 increase in Reynolds number may be possible every 20 years.
2. DNS is limited by computer capability, and therefore will always push the existing capability to its limit. In other words, these computations will only be carried out for basic research on "canonical" flows. Flows in complex geometries such as those found in practical devices will generally not use DNS because of the high overhead in performing the calculation.
3. DNS requires extensive post-processing to enable analysis and the development of real understanding. Significant advances in the manipulation of space-filling data sets are essential for the full utilization of the computation. Based on the work by Spalart [14] and Robinson and Kline [15], it seems that at present the time required for analysis exceeds the time required to perform the calculation by about a factor of three (it took about a year to run

the code, and about three years to analyze the data, to do at least partial justice to the data set).

It is interesting to note that very similar limitations apply to experiments which provide three-dimensional, time-evolving data. The resolution difficulties are very similar, and the post-processing needs are identical. Any development which aids DNS is likely to help the experimentalist in equal measure, and thus we can expect to see that as DNS grows in scope, the experiments will match the computations in complexity and detail. Most probably, the experiments and the computations will develop together, each taking advantage of any technological breakthrough, and both limited by the contemporary level of that technology, without one necessarily superseding the other.

It seems inevitable that for the more complex flows, where complex geometries and complex physics are present, the DNS and PIV approaches will simply not be sufficient. What are the alternatives? For computation, there are two possibilities: LES, and turbulence modelling based on the Reynolds-averaged equations. In either case, experimental work becomes essential.

In LES, the equations of motion for the large-scales are solved completely, and the effect of the small scales on the large scales is modelled, usually by some version of an eddy viscosity transport coefficient. Essentially, the size of the grid determines the cut-off between the subgrid and the resolved scales. The increase in the Reynolds number that can be obtained by LES compared to DNS depends on the accuracy of the sub-grid scale model, and the treatment of the near-wall flow. For the case of a fully-developed channel flow, a factor of 10 increase in Reynolds number is probably conservative, and Karniadakis [38] has recently suggested that factors of 100 to 1000 may be possible, depending on the quality of the subgrid scale model, the nature of the output required, and its level of accuracy. Further substantial improvements seem possible if the wall layer could be modelled in an acceptable way [39]. Here, the roles of experiments and theory are crucial in providing the detailed data required to develop an accurate sub-grid scale model, as well as an accurate wall-layer model. Of course, DNS can also be used in this effort, and a strong interaction between experiment and computation appears to be the most fruitful approach.

So will LES cure all ills? Should all of our shrinking experimental resources be devoted to the development of subgrid scale models and wall-layer models? It seems unreasonable to do so, not because the promises held out by LES are unrealistic, but because this approach ignores the wider possibilities of experimental work.

First, turbulence models will still be necessary for engineering calculations and design. These models will continue to require fairly traditional experimental input, such as correlation measurements.

Second, new physics will require new understanding, and this understanding will come more quickly by experiment (examples: curvature, rotation, compressibility, buoyancy effects, stratification). For example, we are still a long way from even contemplating an LES solution to any shock wave boundary layer interaction problem at any Mach number (except perhaps at very low Reynolds number). In such areas, experiments are still the only source of information, and with new experimental techniques such as Rayleigh scattering, great progress can be made (see, for instance, [40]).

Third, the calculation of very high Reynolds number flows will require experimental input on Reynolds number scaling.

Fourth, the proper use of initial and boundary conditions in the computation is only beginning to be understood, and numerical and physical experiments will be necessary for further progress. (Examples: the use of periodic boundary conditions, the proper initial conditions for studying free shear layer and boundary layer transition, setting boundary conditions for studying free shear layer growth, especially at high convective Mach numbers, the persistence of initial conditions further downstream, prediction of flight transition Reynolds numbers.)

Fifth, "accidents" and parametric studies play an important role in discovery and invention. There exist numerous examples in science where a lucky accident or the Nth variation on a parameter (where N is a large number) has led to a major breakthrough in solving a complicated problem. While they do not seem to be as rational or inspired as we might like, they are a legitimate part of experimental work. Examples outside the field of fluid mechanics include the discovery of penicillin and the development of the electric light bulb. A good example in fluid mechanics is the discovery of drag reduction by the use of riblets.

Which Experiments?

Which experiments should we be doing? It sounds like a simple question, yet the answer turns out to be very difficult. During the Princeton meeting, a considerable effort was made to formulate a reasonable answer, at least for turbulence research. What are the big questions? Why are they important? Can we distill the community-wide efforts and define it in terms of a few (not too nebulous) thrusts? Can we plan for the future, or is this foolhardy? Are we conducting a coordinated effort, or are we responding to short term needs and objectives?

The answer is crucial in that it defines the aims of the turbulence community: unless we can define our role, we cannot hope to maintain our current strength as a community, never mind grow. Professor Brown of Princeton stressed that research in turbulence should be driven by important applications, such as vehicles (forces, moments, aeroelastic problems, heat-transfer issues, noise control), engines (heat addition and heat transfer, work extraction and work done on the gas, emissions), the environment (water resources, emissions, atmospheric and oceanic circulation systems, heat transfer, micrometeorology), and materials processing and manufacture (energy addition and extraction, mixing, reactions, emissions, forces). He then identified important areas which follow from the technology drivers, and which require better physical understanding (and therefore can be used as a guide for future experimental work). Specifically, he suggested high Reynolds number flows, mixing and combustion, heat transfer, flows with unsteady boundary conditions, three-dimensional separation, and flow fields where the mean vorticity is neither parallel nor perpendicular to the velocity.

A small working group headed by Prof. Knight of Rutgers took a similar approach in that they made a distinction between fundamental questions in turbulence, and important examples or applications of turbulence. The fundamental questions are by definition very basic, but they have a crucial bearing on the understanding of the applications. Examples of these questions include coherent structures, effects of compressibility, entrainment, kinematics versus dynamics, local isotropy, and Reynolds number invariance. In the applications of turbulence, six major categories were identified: combustion and mixing, complex flows, control of turbulence, compressible turbulent flow, geophysical fluid turbulence, and turbulence modelling.

If we are satisfied that these are the major problems then another important question arises: do we have the facilities and resources available to carry out the experiments required to study these problems? For example, do we have the facilities to study the effects of Reynolds number, Mach number, Rayleigh number, etc., or do we need new ones? It would seem that for certain kinds of problems, new facilities will be essential. For example, it is clear that new facilities will be required to study Mach number effects on turbulent boundary layers, if a wide range of Reynolds number is required at the same time. In fact, for this particular work, some facilities already exist, but they will almost certainly be unavailable for this kind of fundamental study. A recent study on Physics Through the 1990's commissioned by the National Research Council [41] devoted some space to the physics of plasmas and fluids, and its findings echoed many of the issues raised in this paper (see Appendix A for a summary of its findings and recommendations). In particular, it noted that "Many unique national experimental and computational facilities are not readily available to a large proportion of the research community," and it recommended "the provision of funds and organizational mechanisms to make unique national fluid-physics facilities available to the university and nongovernment communities for basic research." No matter what arrangements can be made to use existing facilities, it is clear that a number of new facilities must be built, and adequately equipped. While this fact is widely recognized, there are currently very few resources available, either for facilities or instrumentation.

Where Will The Resources Come From?

Limited funding has always been a source of contention in fluid mechanics research, almost by definition: it is difficult to envisage a level of funding where people in the field would admit to having enough. Nevertheless, it is also true that funding in fluid mechanics research has been declining in recent years. NSF decided to reduce its support in this area, having expressed the view that fluid mechanics is a "mature science"; NASA reduced its level of university support, in response to its own internal budget difficulties; and the level of support from DOD agencies for university research has not held its own within the total 6.1 budget.

We could take the view that these reductions reflect the true importance of the field, and that there exist more important areas of research which legitimately deserve a greater share of the pie. We could point to the increasing popularity of the APS Fluid Dynamics Division meetings (contributions are up from about 300 in 1981 to about 720 in 1990) as an indication that there are maybe too many people in the field, and that some scaling back could be healthy. We noted before that the percentage of contributions at the APS meeting which were experimental in content decreased from 44% to 36% from 1981 to 1990. However, the absolute number of experimental papers increased from 134 to 257 (which prompts the question of how these studies are funded, given that the resources have actually decreased). Do these numbers indicate that there is already too much effort in this area? Or is it true that fluid mechanics plays such a pervasive role in environmental and technological processes that even a much greater effort can be justified? Personally, I lean towards the latter view: it is relatively easy to list many critical technologies where fluid mechanics and turbulence play a decisive role, as we did at the Princeton meeting, and it is just as easy to demonstrate our limited understanding of the field.

It is also clear that if we are to exploit advances in technology to improve our understanding of the field, it will take considerable resources. The difficulty in persuading funding agencies to increase their support, or even maintain their support at current levels, stems from the

fact that fluid mechanics is often only one component of any given application, and that as a study in its own right it does not have the recognition it should have. It has no national visibility whatsoever.

And we, as a community, carry some of the responsibility for that lack of visibility. We do not act as a community, preferring to identify ourselves with one or two of many different professional societies, or acting as a lone individual. In fact, at a time where resources are shrinking it is difficult not to feel personally under siege, and cooperative, community-wide efforts may actually seem to threaten the welfare of our individual efforts. We should also remember that the experimental, numerical and analytical branches of fluid mechanics depend on each other for our continued good health, and that the problems facing the experimental effort are problems for the entire community.

There is no question in my mind that the future good health of the fluid mechanics community depends on our ability to respond to the strategic technological challenges summarized earlier. We, the academic research community, need to think more in terms of the application of our research, and to communicate its relevance more effectively than we have done in the past. For this to be a credible process, we need to become much more familiar with the problems facing industry than we currently are, and we need to develop an interface between the research community, and the funding agencies. This is not to be mistaken as a call for more applied research, far from it. Earlier, we made a distinction between fundamental questions in turbulence, and fundamental applications of turbulence. Progress in developing applications can only come from finding answers to fundamental questions. On the other hand, to find the resources necessary to study the fundamentals we need to be able to show how our work relates to industry and to national priorities. So there are two major challenges facing our community: the need to build bridges between industry and academia, that is between fundamental research and its application, and the need to raise the visibility of fluid mechanics and turbulence as a national priority.

Building bridges can be done on any scale. For example, on the national level, the National Center for Advanced Technology (NCAT) was established by the Aerospace Industries Association of America in 1989 to:

- Develop national consensus and support for key technologies (which include air-breathing propulsion and rocket propulsion).
- Support adequate and stable funding in the federal budget for an adequate technology base and also for specific key technologies.
- Utilize industry and government to adopt key technologies development plans as their strategic research and development plans.
- Provide counsel to government departments, agencies and others regarding technology integration, planning and policy.
- Act as an impartial bridge between industry, the administration and congress to encourage adequate and continuous support of all technology-related resources, such as manufacturing processes, testing and evaluation, and the education of science and technical personnel.

The emphasis on stable funding, and continuous support is particularly interesting.

Small scale efforts can also be very productive, and a good example drawn from the applied mathematics area is the Workshop on Mathematical Problems in Industry, run annually by the Rensselaer Institute for Applied Mathematics. The aim of these workshops is "to interact with industrial researchers in order to identify, formulate and analyze mathematical problems originating in industry, as well as carry out research in related areas of mathematics" (quotation taken from the announcement). The first day of the workshop is devoted to

problem presentation by the industrial participants, while the remainder of the week is spent working on specific problems in small informal groups. These workshops have been very successful by any measure, and the seventh in the series will be held this year. They seem to provide a proven model for encouraging similar industrial/academic interactions in fluid mechanics, or heat transfer.

To develop a greater visibility, several avenues seem worth exploring. The 1986 NRC study [41] has already been mentioned. By itself, a National Research Council study can have considerable impact in setting budget priorities and raising the level of awareness. But interestingly, it noted that "Support for basic research in fluid physics comes from a wide variety of sources. This is both a strength and a weakness, but the field suffers from a lack of an individual national identity," and it recommended "that a mechanism be established to provide a continuing survey of research support in fluid physics vis-a-vis the field's national and intellectual needs." The concept of a group to represent the interests of the community is indeed a valuable one, and some efforts are currently underway to try to establish such a group. Individual efforts can also be successful, and we may have to accept such an advocacy role as part of our job description.

Initiatives in Instrumentation

Over the next ten or twenty years, the impact of experimental work on turbulence research will be dictated largely by the development of new instrumentation. We have already seen the important role played by new instrumentation in recent years: the development of field-based methods to obtain data in two and three dimensions has had an enormous impact on our understanding of turbulence. As these techniques evolve, they will improve their spatial and temporal resolution, the cost and complexity will be reduced, and their application will, in a sense, become routine.

A good example is the development of the RELIEF technique for marking time lines in supersonic turbulent shear layers. The initial experiments [42] were performed using three lasers: the tagging required a pair of synchronized, high power pulsed lasers separated by the vibrational energy of the oxygen molecule (a Nd:YAG/dye laser system injection locked with a single frequency diode pumped Nd:YAG laser), and the interrogation required an injection-locked ArF excimer laser operating at 193 nm. The two tagging lasers needed to be aligned and focused to the same point in space, as well as being synchronized in time. The system was expensive, difficult to align, and very sensitive to external disturbances. A new system is therefore being developed, where a single Nd:YAG laser was quadrupled into the UV (256 nm), passes through a Raman cell to obtain the required frequency shift, and the interrogation is performed using a flashlamp. The difficult alignment procedure has been eliminated, and the system is robust, and relatively inexpensive (about the cost of a two-channel LDV system). Preliminary tests have shown that the new system gives results equal to the earlier system.

We can expect that similar developments will help to put PIV and LIF within the reach of any research group in fluid mechanics (see [32] for some recent advances in this direction). With the continued decrease in the cost of computers, lasers, and video processing equipment, I feel confident that experiments will be able to match the progress forecast for the computational effort, and sometimes exceed it.

I do not think that experimental work will get cheaper: just as is true in the world of CFD, the front-line work will always push the limits of the available technology, and by definition

that will keep the cost high. But there will be a corresponding payoff in the improved quality and scope of the data.

I also believe that these improvements will not happen without a concerted effort. The 1986 NRC study summarized in Appendix A [41] noted the "remarkable growth in non-intrusive laser-based flow diagnostic techniques," and recommended establishing "a targeted research initiative to investigate and develop instrumentation for essentially simultaneous multipoint measurements of flow properties throughout large volumes." Now, new laser-based instrumentation is usually based on a creative understanding of applied physics which is not usually found among experimentalists, who generally have an engineering background. What seems to be required are collaborative efforts among people in fluid mechanics and people in applied physics and instrumentation, working together to solve a specific problem in fluid mechanics (or heat transfer, or thermodynamics). This is in contrast to past efforts in funding instrumentation, where instrumentation development usually occurred without a direct connection to a specific problem. To make this kind of interactive effort possible, new funding initiatives in instrumentation will be necessary, as well as a restructuring of graduate training in experimental science. I believe these initiatives to be essential for the continued good health of experimental fluid mechanics research.

Closing Remarks

Many issues have been raised in this paper, but I cannot pretend that my perspective is broad enough to encompass all aspects of experimental fluid mechanics research, or even turbulence research, and my thoughts on the field of heat transfer can only be regarded as superficial. Nevertheless, I hope that I have conveyed my belief that fluid mechanics is still a very exciting area for an experimentalist, and that the future opportunities look even brighter. To quote the 1986 NRC study again: ".....fluid physics remains intellectually stimulating because of the natural occurrence and importance of its problems. In addition, new levels of understanding of complex phenomena have further vitalized this field. Much of this understanding has been created by the development of new tools that enable us to attack the nature of complex phenomena that hitherto have appeared intractable mysteries. Thus, the study of turbulence, complex high-speed flows, biological flows, and geological phenomena has been paced by new developments in powerful computational and instrumentation techniques. We look forward to the next decade as a time of excitement, adventure, and discovery. The associated implication for the mastery of many important practical problems so necessary to the well-being of our nation and the world serve as a further stimulus." [41]

I feel that these thoughts apply equally well to the experimental, analytical and computational aspects of fluid mechanics. Clearly, the most effective progress will be made when the researchers in these three areas interact and collaborate in a non-competitive atmosphere. However, to maintain a reasonably equitable partnership, the experimental community may have to be more focused than it has been in the past. In particular, it needs to develop a sense of identity and purpose, something it seems to lack right now, and it needs to present its views in some coherent way. Only then will the resources become available to carry on the research, develop and apply new instrumentation, build new facilities, and tackle new problems.

Acknowledgement

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Appendix A

A recent study on Physics Through the 1990's commissioned by the National Research Council [41] devoted some space to the physics of plasmas and fluids. In summary, it noted that:

- * "Support for basic research in fluid physics comes from a wide variety of sources. This is both a strength and a weakness, but the field suffers from a lack of an individual national identity."
- * "Many unique national experimental and computational facilities are not readily available to a large proportion of the research community."
- * The increasing "importance of the computer with applications that range from the rapid organization of data and their subsequent analysis and display all the way to the direct numerical simulation of the major features of some turbulent flows."
- * The "remarkable growth in non-intrusive laser-based flow diagnostic techniques."
- * The "low emphasis on fluid physics in our physics curriculum..."

The study recommended

- * "that a mechanism be established to provide a continuing survey of research support in fluid physics vis-a-vis the field's national and intellectual needs."
- * "a targeted research initiative to investigate and develop instrumentation for essentially simultaneous multipoint measurements of flow properties throughout large volumes."
- * "the provision of funds and organizational mechanisms to make unique national fluid-physics facilities available to the university and nongovernment communities for basic research."
- * "the expansion of the role of NSF in supporting basic fluid-physics research, with particular emphasis on the support available for basic fluid-physics research related to engineering science."
- * In education: "enhancing collaborative interdisciplinary relationships between the physical and engineering sciences" in the undergraduate curriculum, updating teaching laboratory equipment, and maintaining the fundamental importance of analytical methods.

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APPENDIX C
Advocacy Letter



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Re.: Proposed Ad Hoc Planning and Advocacy Group for Turbulence

Dear Lex:

The present time is an exciting one to be involved in turbulence research. For years, it has been fragmented into many communities such as physics, pure and applied mathematics, different branches of engineering, and geophysics; moreover, it has meant different things to different people in different contexts. These diverse communities with their diverse ideas now seem to be drawing closer together than ever before, to form a common front addressing the key questions of turbulence. In order to facilitate this unification process, as well as to assess the present and future prospects for the field, we believe that it would be useful to collect a group of interested people to discuss issues related to the intellectual challenges that face us, as well as to the financial resources required to enable sustained and perhaps elevated research efforts; to discuss whether it would be worthwhile to explore mechanisms by which the broadly-based turbulence community could act collectively and enhance communication to assure appropriate funding levels to address the *basic* issues of turbulence in a more stable, broadly-based framework.

In order to discuss these issues several meetings have taken place during the past year. Most of the participants are well-known to the community: G. Brown (Princeton), D. Bushnell (NASA), J. Gollub (Haverford), C.-M. Ho (USC), A.K.M.F. Hussain (Houston), D. Knight (Rutgers), S. Lekoudis (ONR), J. L. Lumley (Cornell), J. McMichael (AFOSR), R. Miles (Princeton), H. Nagib (IIT), S.A. Orszag (Princeton), W. C. Reynolds (Stanford), A. Poshko (Cal Tech), A.J. Smits (Princeton), K. Sreenivasan (Yale), and J. Sullivan (ONR). A consensus was formed to ask me to serve as Chair during the initial period. The views I express in this letter are majority views, but not unanimous.

It was felt that an appropriate first step would be the formation of a group of researchers who would seriously consider the various issues involved and act as an advocacy group for turbulence research. This Group would be called the Ad Hoc Planning and Advocacy Group for Turbulence. It would be responsible for interacting with the broader research community and for gauging its response. It was felt that good science should be international, and that close liaison should be maintained with Europe and the Pacific rim. While this Group would exist outside the various interest groups in the professional societies, these groups would be represented and would be kept informed. Representatives of the funding agencies indicated that they would strongly encourage the formation of such a group and that they would welcome the opportunity to interface with such a group in the future. We, and they, feel, however, that they should not be involved in the group. A concurrent step would be the formation of a loose federation of several currently existing centers (or groups) of research workers who would make efforts to communicate with each other regarding the direction of the future of the field, both to strengthen the field and to foster interactions on issues of common interest.

During these initial meetings, a number of topics were discussed. We summarize them here to indicate the drift of our thoughts. This might be regarded as no more than a tentative agenda for early meetings of the Advocacy Group. There was unanimous agreement that greater visibility for the field is essential; unless the current excitement of the subject is communicated to others (ranging from undergraduates to the general public and congress), maintenance of even the present scale of activity will be difficult. This suggests a carefully orchestrated publicity program. With increased visibility and public and congressional awareness, a larger-scale focused activity might be sustainable. The strong sentiment was expressed, however, that there is no single activity in turbulence research which could justify such a strong focus, as a result of the varied research emphases and diversity of issues involved.

The group seriously considered the possibility of large-scale discussion and coordination of research planning, funding and administration. In addition, the possible designation of pilot centers (as they are called in the European turbulence/combustion consortium ERCOFTAC) was suggested, where specialized equipment, data and/or expertise exists, and to which formal visitor access could be arranged through the Group.

The group discussed at some length the possibility of a national center for experimental research in turbulence: whether there is a need for it, what form it could take, and what might be the serious intellectual problems that could be resolved by creating such a center. For comparison, the National Center for Atmospheric Research and the Combustion Research Facility at Sandia were mentioned. The comparison was felt not to be particularly apposite, since in both cases the communities involved are much larger. Parallels were also drawn with collaborative efforts already begun in Europe (ERCOFTAC) as well as in Japan. The group members emphasized from the beginning that such a center, if it were to be created, should be dedicated to basic research. The analogy to high-energy particle physics was thought to be also inappropriate for many reasons, primarily the entirely different demographic weight of the turbulence community in the society as a whole, but partly because of the applied nature of turbulence and the intrinsic fragmentation already mentioned.

In at least one respect a center with one or several large and well-designed facilities could be justified (because one well-carried-out experiment at high Reynolds number would be worth more than many compromised experiments), but many elements would have to be considered carefully before contemplating such a step. For example, much consideration must be given to the compelling scientific issues; how would their resolution benefit society at large? How could a consensus within the community be created in order to support such a broad effort? What effect would the center have on the type of research carried out, and on the mode of working? What can one learn from existing centers of activity? There are also more technical questions, such as the nature and limitations of available instrumentation. For example, what are the prospects for making well-resolved measurements in three-dimensional space and time (since it was generally agreed that single-point measurements are not sufficiently informative)? Finally, the question was raised as to whether theory has given sufficiently clear and general guidance to serve as a base for a large-scale experiment.

I write you now to ask for your support for the formation of such an Ad Hoc Planning and Advocacy Group for Turbulence. If you support it, I ask you to suggest the names of individuals that you think would be appropriate members of the Group. We feel that the various organizations that are involved in turbulence research, such as APS/DFD, ASME, AIAA, AIChE, AMS, SIAM and many others should be asked to designate representatives to the Group. Which organizations do you feel must be represented? Can you suggest people who are at once members of the turbulence community and of these interest groups who might represent these organizations? Once constituted, the Group will meet to organize itself and elect an executive committee and officers, since it is clear that a smaller working group is essential for decision making.

The issues suggested above are no more than a taste of what might be discussed. We welcome your thoughts and input on these and any other issues that you feel are significant. We look forward to a broad and active discussion of these issues throughout the research community.

Very truly yours,

Chris T. Funkhouser

JLL/gf